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Archaeology in Nigeria: an official report

WE have frequently referred in our Editorial Notes to the state of archaeology in countries for whose government and cultural facilities the British Government is responsible, contrasting its attitude with that of some other countries. It seems therefore opportune to reprint in full (with the permission of the Nigerian Government) the 'Annual Report of the Antiquities Section for the year 1946', which is the latest to reach us. It is signed by Mr K. C. Murray, Surveyor of Antiquities, over the date 20 January 1947. Need we add that, in thus reprinting his Report, we hope to strengthen the hands of Mr Murray and those others who are struggling to establish a proper Department of Archaeology and Anthropology in Nigeria?—EDS.

In July 1943 a member of the Education Department was seconded to the Public Relations Office in order to tour and make a report on the practical steps to be taken to preserve the known antiquities of Nigeria. During the following twenty-two months this officer travelled extensively in Nigeria paying particular attention to the antiquities at Ife, Benin, Esie, Ikot Ekpene, Bende and Oron. Many of the chief centres in the Northern Provinces at which antiquities had been reported were also visited.

In 1946 Antiquities and Cultural Relations were constituted as a separate section of Government and were for the purposes of the Estimates placed under the Secretariat. A new post of Surveyor of Antiquities was established.

Early in 1946 Mr H. J. Braunholtz, the keeper of the Department of Ethnography in the British Museum visited West Africa in order to survey the situation as regards the preservation and display of antiquities and to report to the Governments concerned such recommendations as might seem called for. His visit was financed by a Research Grant under the Colonial Development and Welfare Act. He spent six weeks in Nigeria, and accompanied by the Surveyor of Antiquities, visited, among other places, Abeokuta, Jos, Umuahia, Ikot Ekpene, Oron, Owerri, Awka, Benin, Ikerre, Esie, Ilorin, Ife and Kano.

Besides this tour with Mr Braunholtz, the Surveyor made three prolonged tours during the year. On the first he visited Maiduguri, Ngala, Mubi, Biu, Yola, Jalingo, Kombo, Tula, Bauchi and Jos. The second was made mostly by canoe and launch through the creeks between Epe and Siluko. Here there are many ancient head-dresses which mostly represent water-spirits, large drums belonging to the Oshugbo Society, and sundry old objects of bronze of varying importance, chief among which are two masks of Benin style at Ugbo. The third tour was made to the Eastern Provinces, to Umuahia, Abak, Etinan and Aro District, all of which have their own types of traditional carving.

At Aro Chuku a suggestion was followed up that a small museum for its historical objects should be established, but insufficient local support is forthcoming to make this an immediate recommendation. Another purpose of the visit to the Aro District was to search out carved head-dresses covered with skin. Examples from the Cross River are among the finest carvings from West Africa in European museums, but unfortunately the art seems to be dying out and the surviving examples are rapidly getting ruined.

During the tour in the Eastern Provinces the Surveyor met the Oron Clan Council and once more got their confirmation that they want a museum to be built at Oron for their ancestral carvings. Meanwhile many of the surviving carvings are lying decaying in the open.

On this same tour work was done in the Western Provinces. A fairly close survey was made of antiquities at Owo in Ondo Province. Here the people gave their utmost support and a number of valuable objects of wood, ivory and brass were listed. Some days were also spent on a preliminary survey at Agbor and at places in Kwale Division to find out what kinds of antiquities occur in those parts. Kwale, at least, will need a more extended visit and various wood carvings and terra-cottas need steps taken to preserve them in the villages.

The preliminary survey of Nigera, which is not yet completed, has shown that there are a great number of objects that will have to be preserved where they are now. These include drums, doors and posts of buildings, masks, metal objects, heirlooms of the Northern Emirs and historical ruins such as those of Surame in Sokoto Province. It would be hard to exaggerate the pace at which the destruction of Nigeria's antiquities goes forward, and trained staff is needed if these things are to be saved. For many of them, ten years from now will be too late. A little is being done with the help of District Officers ; thus some of the surviving slit-drums in Aba Division have been placed on cement stands and they therefore should run less risk from damp and termites. Boxes are being provided for some small objects.

No progress with the building of museums has been made during the year. Money has been provided for two years for a museum at Benin, but the site has not been finally chosen, nor its plan decided. Meanwhile a small room belonging to the Native Administration is being used as a temporary museum under the honorary curatorship of Chief Egharevba, the Bini historian. Six well-designed glass cases, made by the Public Works Department at Benin, have been put in the room, and in them have been collected all the bronzes that have hitherto lain in a rather dirty and neglected state in the Iwebu Court of the Palace. All the objects in the museum have been given a preliminary cleaning, have been fully catalogued and have labels in the cases to describe them. The Oba has generously put in this temporary museum a number of other objects from the Palace, including a fine bronze head which had disappeared from view for over twenty years. Several of the Benin chiefs have also lent things. This collection, which is well on the way to overfill the room, has attracted much interest among the people of Benin and visitors to the place, for none of the exhibits had previously been on public view. There is, in particular, a small bronze figure of a man wearing robes and necklaces that has the appearance of being Ife work.

The same report of no progress must be made of the Ife Museum. Six years ago the Carnegie Corporation granted £1,000 towards the museum and later had to withdraw it as it was not used. Money has been placed in the Estimates for the last two years and at various times promises have been made that someone will be sent to supervise the building, but owing to shortage of staff it has been found impossible to spare anyone, and the museum remains unstarted. In the meantime two glass cases originally destined

for the Exhibition Centre in Lagos have been placed in the small room in the Ife Council Hall where the world-famous bronzes and terra-cottas are kept. Although these cases are not altogether satisfactory they will at least lessen the risk from damage from handling by visitors. All the objects have been catalogued in detail and on each of his visits to Ife the Surveyor has added to the list that was made in 1943 of antiquities and sites. On the recommendation of Mr Braunholtz and with the co-operation of the British Museum the bronze heads at Ife are being sent to London for cleaning, for they, like all other ancient bronzes in Nigeria, are suffering from active corrosion.

Throughout the year purchases have been made and gifts from both Africans and Europeans have been received for the future Nigerian Museum. The labelling and cataloguing of over 250 articles that have been added to the Government collection has been kept up to date and continual care has been successful in preventing damage from insects. Lithographed forms of thanks have been made for giving to donors to the museum. Almost the entire collection has had to be kept in the Surveyor's house and for that reason it has been impossible to collect as much as could and ought to have been collected. The Old Golf Club on the road to Ikoyi has now been made available for a Museum Store and when some alterations have been done the problem of storage should be solved for several years.

It is unlikely that it will be possible to exhibit things in the Old Golf Club. For that the Exhibition Centre must be used and it is proposed that temporary exhibitions should be arranged there from time to time. One such exhibition, the first exhibition in Nigeria of Nigerian antiquities, was held in the Centre at the end of the year. It was opened by the Acting Commissioner for the Colony and during the four weeks that it was held was visited by over 30,000 people. The idea that Nigerians are not interested in their arts and history was therefore disproved.

Contact has been made with various sources for which it may be possible to get objects for the Museum. Thus in this country the Police have arranged to make available artistic and ethnographic material that they or the Courts confiscate. Notes on antiquities have been contributed to the local press in order to arouse attention to the need for preservation and information about owners of Nigerian Antiquities in other countries is being collected. Through the generosity of Major Lugard, important articles were received in 1945 from Lord Lugard's collection. Some of the biggest collections of Nigerian antiquities, particularly of Benin bronzes were in Germany: the Nigerian Government set aside £25,000 in the 1946–47 Estimates for the purchase of Nigerian works should any become available. It is hoped that some of the Ife bronzes taken surreptitiously from the country in 1938–39 may be recovered.

It is unfortunate that archaeological work in Nigeria is neglected for the whole science of prehistory in Africa is suffering. The tin-mining activities on the Plateau provide splendid opportunities for securing information about the Stone Age in Nigeria, but the evidence is in danger of being lost. It was therefore very fortunate that Mr B. E. B. Fagg, Assistant District Officer, and a trained archaeologist, has happened to be stationed at Jos. The fact that the tin-miners have known of his interest has meant that all finds that take place have been brought to his notice and as far as possible he has been able to make careful records. During his local leave in 1944 he discovered and excavated a rock shelter at Rop and disclosed there an important microlithic industry which linked up with a similar industry that was being excavated at about the same time on the Gold Coast. Funds were provided so that he could do further work during his local leave in 1946. Two rock shelters and four sites in the alluvial deposits of the Nok valley, where some important pottery figurines had been found, were examined. (*See*

' Africa ' January, 1943, Bernard Fagg : A Preliminary note on a new series of pottery figures from Northern Nigeria). Later in the year an important palaeolithic site was found at Forum.

Mr Fagg, who has been Secretary of the Jos Museum Committee, has been looking after the archaeological and enthnographical collection at Jos. There has been nowhere to show this collection since it was ejected first from one, then from another home, during the war. It has now been packed in strong cases and put in a special store. A proposal has been made and plans have been prepared for a museum at Jos, costing between £15,000 and £20,000, that will serve the surrounding area up to a radius of about eighty miles.

Throughout most of the year vacancies for staff have been unfilled. No suitable candidate for the post of Technical Assistant was found and it seems probable that special training must be provided before one can be appointed. As no typewriter has been obtainable it has not been worth while to appoint a clerk. The Public Relations Officer has kindly allowed the Surveyor when in Lagos to make use of his office and staff.

Plans and decisions on general principles and future policy to be followed in the protection of antiquities in Nigeria await the arrival of Mr Braunholtz's report. One of the chief questions is whether there should be a central museum, in which the more important objects will be concentrated, and small regional museums, with touring collections to serve provincial headquarters and educational centres.

Notes on Minoan Chronology

by R. W. Hutchinson

MOST statements on the subject of Aegean chronology depend and will continue to depend on the Minoan system evolved by Sir Arthur Evans, who based his absolute dating chiefly on synchronisms with Egypt according to the chronology laid down by Dr Eduard Meyer (1).

This dating for the Bronze Age in Crete runs as follows :—

E.M. I	=	3400–2800	M.M. III	=	1700–1550 (2)
E.M. II	=	2800–2400	L.M. I	=	1550–1450 (2)
E.M. III	=	2400–2100	L.M. II	=	1450–1375
M.M. I	=	2100–1900	L.M. III	=	1375–1100
M.M. II	=	1900–1700			

This system allows thirteen hundred years for the Early Bronze Age, a very long time for the cultural developments that took place therein. We must not, however, reject this long chronology if it is established by definite synchronisms with historical dates in Egypt, Asia Minor or Mesopotamia, countries whereof we possess written records for these times.

It will, however, be generally admitted, I think, that the Minoan synchronisms with historical cultures such as that of Egypt are very incomplete for the periods before 1700 B.C., and some revision of the Early Minoan dates would seem to be demanded to agree with that now required for Meyer's Egyptian chronology (3), and with the shorter chronology now generally accepted in one form or another for Mesopotamian history.

Since the publication of the Early Minoan material the Egyptian finds from Knossos have been discussed by Dr G. A. Reisner. He states that the Pre-Dynastic round-bottomed jar (his type IIIa) has up to date not been found in Crete (or on the mainland we may add). The spheroidal jar with flat or disc base (Reisner's Type IIIb) extended according to Reisner from Dynasty I to Dynasty IV (4). A hornblende porphyry bowl of this kind (5) and the fragment of another (6) were found at Knossos in an unstratified deposit north-west of the Palace.

A degenerate form of this jar with unpierced handles (Reisner's Type IIIc) appeared in Egypt during Dynasty III (says Dr Reisner) and was more characteristic of IV and V. A fragment of such a jar was discovered at Knossos between a late neolithic and a sub-neolithic stratum under the South Propylaeum (7) and associated with it was a base probably belonging to a similar vase. Two fragments that belong either to III B or III C

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[1] *Aegyptische Chronologie.*

[2] For the latest revision of L.M. I and II dates here followed see Evans' *Palace of Minos*, II, p. 7.

[3] *Die Ältere Chronologie Babyloniens, Assyriens und Ägyptens.*

[4] Reisner, Antiquity, June 1931, p. 200, 9 ff.

[5] *Palace of Minos* II, I, p. 30, fig. 12 ; Pendlebury, loc. cit. p. 21.

[6] *Palace of Minos* II, p. 31, fig. 28 ; Pendlebury, loc. cit. p. 21.

[7] *Palace of Minos* I, p. 65, fig. 31.

jars were found associated with the III B vases mentioned above from the unstratified deposit north-west of the Palace.

Among the most important of the Egyptian stone imports were the two fragments found in Neolithic House A (**8**) at Knossos.

One of these fragments was the base of a cylindrical jar which might have belonged to any of the first six Dynasties but was certainly not Pre-Dynastic; the second was a fragment of a III B or III C bowl. Even if we assume the second to be part of a III B vase we have evidence here, if Reisner's dates are trustworthy, that the Cretan neolithic overlapped with the Early Dynastic period in Egypt, that is to say E.M. I must begin later than 3197 B.C.

Of the vase under the South Propylaeum Dr Reisner says 'This jar is of the type form III C and the most reasonable date is Dynasty III, but it might be of Dynasty IV or V'. This would imply that the Cretan Neolithic must have overlapped with the opening of the Third Dynasty in 2778 B.C. approximately.

Unfortunately Egyptologists do not all agree on the question of the stone bowls. My friend the late Captain J. D. S. Pendlebury was of the opinion that Dr Reisner had overstressed the latest appearance of a type and not sufficiently allowed for the survival of individual vessels as heirlooms. The second argument is of course rather double-edged since the bowls might also have survived as heirlooms in Crete. The Cretan deposits wherein they occur cannot be earlier than the bowls but might be considerably later.

The III B bowls from the area north-west of the Palace at Knossos, Pendlebury would assign to the Middle Pre-Dynastic period (sequence dates 45–54) and the III C bowl of syenite found under the South Propylaeum to the First and Second Dynasties.

We may therefore suggest two alternate schemes of chronology based respectively (1) on Reisner's and (2) on Pendlebury's dating for the bowls.

(1)	E.M. I	=	2700–2500 B.C.	(2)	E.M. I	=	3100–2700
	E.M. II	=	2500–2300 „		E.M. II	=	2700–2400
	E.M. III	=	2300–2100 „		E.M. III	=	2400–2100

I believe that these two schemes give us at least our chronological limits (provided that Professor Meyer's chronology for the Egyptian Dynasties is correct (**9**). I do not think that E.M. I can begin earlier than 3100 or later than 2700 B.C.

The tholos at Marathokephalon in the Mesara contained pottery of all three E.M. periods and some imported objects from Egypt, a circular seal and a scarab both dating according to Pendlebury from the First Intermediate period (approx. 2270–2100) were found at Aspre Petra (**10**).

The large tholos tomb at Hagia Triada also contained Early Minoan pottery associated with an Egyptian scarab of the First Intermediate period, a circular seal dating from the earlier part of the 12th Dynasty and an ape amulet in lapis (also 12th Dynasty). Again we are confronted by the fact that the tomb was probably open for a considerable time, but the cumulative evidence for regarding the period E.M. III–M.M. I as approximately parallel with the 12th Dynasty in Egypt may be strengthened by the evidence from Gournes on the north coast of Crete (**11**) where Dr Hazzidákis excavated some burial enclosures. The finds included besides Early Minoan pottery and

[8] *Palace of Minos* II, pp. 16 and 17, and fig. 6.

[9] I have not considered the alternative chronology put forward by Sir Flinders Petrie, but see *Camb. Anc. Hist.* I, p. 166 ff, and the bibliography on p. 629.

[10] Schaeffer, *Ugaritica* III, p. 18, etc., and references cited there.

[11] Pendlebury, loc. cit. p. 29.

seal-stones a string of faience beads of 12th Dynasty type; Grave B at Gournes contained two scarabs, one belonging to the First Intermediate Period (Dynasties VII–X) and the other to the earlier half of the 12th Dynasty (according to Evans).

I should like to stress the string of beads. Isolated beads or scarabs may remain in use for hundreds of years, but a string of beads all of one date is much better evidence and certainly suggests that E.M. III must overlap some part of the 12th Dynasty.

The interesting ivory statuettes from Palaikastro have been classified as Egyptian and 12th Dynasty, but since it is not certain whether they are Egyptian or Minoan I will pass them over (12).

Apart from the examples considered above the chief evidence for inter-relationships between Egypt and Crete during the Early Minoan period comes from certain stone vases of Egyptian types found at Knossos and Mochlos. Stone bowls unfortunately may have been preserved as heirlooms, as were, for example, the Egyptian bowls in Mycenaean graves at Mycenae and Asine on the mainland, and in the Royal Tomb of Isopata near Knossos (13).

A tomb at Mochlos (14) contained a cylindrical marble jar with projecting base and rim associated with Early Minoan pottery. This vase-form was well known in Egypt during the 6th Dynasty and one such vessel can actually be assigned to the year 2486 B.C. by an inscription referring to the Sed festival of Pepi I. Similar vases in limestone were also discovered in Tholos II at Porti (15) (with E.M. III and M.M. I pottery) in the large tholos at Hagia Triada (16) (with E.M. pottery), at Platanos (17) and in the tomb at Marathokephalon (18) (which was used throughout the Early Minoan period). The two last-named sites, as we have remarked, contained objects of the First Intermediate Period and the Hagia Triada tomb also had an amulet of 12th Dynasty date.

These synchronisms suggest that M.M. I must overlap with the 12th Dynasty, E.M. III with the 11th and 12th, and E.M. II with the First Intermediate period, and perhaps also with the 6th Dynasty. It must be remembered, however, that two periods may overlap and yet be by no means parallel.

Since Evans' publication of his scheme of Minoan chronology a mass of material has been published affecting to some extent the relative chronology and to a greater extent the absolute chronology.

In Crete the excavations of the Italians in the Mesara have been published in a final form (19), those of the French at Mallia (20) and of the Greeks in various parts of the island (21) have been illustrated by successive reports and the general position reviewed by the late Captain Pendlebury (22).

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[12] *B.S.A. Suppl.* 1923, p. 125, pl. XXVII; Pendlebury, loc. cit. p. 33; Hall, *Civilization of Greece in the Bronze Age*, p. 273, note.

[13] Mycenae Tomb 518, c.f. Wace, *Archaeologia* LXXXII, p. 223; Pendlebury, p. 57. Asine, c.f. Frodin-Persson, *B.S.R. de Lettres de Lund*, 1924–5, p. 87, pl. XXXIX. Isopata, c.f. Evans, *Prehistoric Tombs*, p. 151 and fig. 128.

[14] Seager, *Mochlos*, p. 80, fig. 47, 3; pl. II, M. 3.

[15] Xanthoudides, *Vaulted Tombs of Mesara*, p. 65 and pl. XXXIXa.

[16] Xanthoudides, loc. cit. p. 65.

[17] Xanthoudides, loc. cit. p. 101 and pl. LIII, 6.

[18] Ἀρχαιολογικὸν Δελτίον IV, p. 19, kg. 15.

[19] Pernier, etc., in *Il Palazzo Minoico di Festos* and articles in *Annuario*.

[20] Chapouthier, Charbonneaux, etc., in *Etudes Crétoises* (*Mallia*).

[21] Marinatos in *Eph. Arch.*

[22] Pendlebury, *The Archaeology of Crete*.

On the mainland of Greece the most important researches of recent years were those of the British School at Mycenae, and the Swedish expedition at Asine, Malthi, and other sites in the Peloponnese (23), and the final publications of the earlier German researches at Mycenae (24), Tiryns (25) and Orchomenos (26). The Cyclades have been rather neglected so far as prehistory is concerned but the Italian excavations on Lemnos, the German on Samos (27) and the British on Chios should be mentioned (28). In Attica prehistoric finds have been excavated by American, Greek and Italian archaeologists.

Further afield Mr Heurtley's explorations in Macedonia (29), Dr Lamb's excavations in Lesbos (30) and Western Asia and those of Dr Blegen at Troy have thrown a flood of light on the connexions between Greece and Anatolia during the Early Bronze Age and later.

The excavations in Cyprus of the Swedish expedition (31) of Dr Dikaios and others (32) have left us a fairly clear picture of the prehistoric culture of that island.

The relations of the Aegean with Syria have been illuminated by the excavations of the French at Ras Shamra (33) and of the British at Tell Atchana (34) in Syria and at Abu Hawam in Palestine (35).

The American excavations in the Corinthia and Athens though primarily concerned with the remains of the classical periods have also uncovered a quantity of material from the various prehistoric periods.

Finally the Mycenaean pottery has been comprehensively treated in the magnificent volumes by Dr Furumark. Professor Marinatos has excavated a number of Minoan sites including the Minoan port of Amnisos and a villa with fine frescoes and the cave of Eileithyia referred to by Odysseus (36), and proposes to attribute the most widespread destruction of Minoan sites to the great eruption of Thera which he dates at 1500 B.C. (37). Dr Platon has excavated a number of sites including an interesting house at Prasses near Herakleion, Dr Theophanides discovered the first Middle Minoan remains in the West. The British School at Athens excavated two tholoi, the first interesting as a structure and the second important because of the 7th century treasure it contained, and a small but rich chamber tomb of that elusive period L.M. IIb.

The Germans conducted a number of excavations during their occupation; the most important Minoan site excavated by them was the E.M. III–M.M. I site and cemetery at Apesokari (38).

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[23] Frodin and Persson, *Asine* and the *Swedish Excavations at Asea, Dendra*, etc.; Valmin, *The Swedish Messenia Expedition, etc.*

[24] G. Karo, *Die Schachtgräber von Mykenae.*

[25] K. Müller, *Tiryns* III and IV.

[26] Kunze, *Orchomenos* II and III. [27] Buschor, *Ath. Mitt.* LX *and* LXI.

[28] E. Eccles, *J.H.S.* 1938, p. 231, and 1939, p. 131.

[29] *Prehistoric Macedonia.* [30] Lamb, *Excavations at Thermi.*

[31] Gjerstad, etc., *Swedish Cyprus Expedition.*

[32] J. Stewart, C. A. Schaeffer, etc.; see *Dikaios Arch.* LXXXVIII, pp. 1–174.

[33] C. A. Schaeffer, *Ugaritica.*

[34] Sir Leonard Woolley, *Antiquaries Journal*, Jan. 1939.

[35] R. W. Hamilton, *Palestine Quarterly.*

[36] Πρακτικά, 1929, 1930.

[37] ANTIQUITY no. 52 (Dec. 1939), pp. 425–40.

[38] T. J. Dunbabin, 'Archaeology in Greece, 1939–45', *J.H.S.*, vol. LXIV; Weinberg's Apekosari is presumably a printer's error.

Unofficial excavations have enriched the collection of Dr Giamalakis with some fine treasures. I understand that many of these, which include a unique bronze moschophoros of L.M. I date, an equally unique neolithic figurine recalling Maltese types but still unmistakably Cretan, a glorious bull's head in gold and many other exciting things, are to be published by the owner's niece Miss Agni Xenaki.

In addition to actual discoveries we have to consider the important monographs published on Aegean materials during the last twenty years. Dr Weinberg gives an excellent bibliography of these for the Neolithic and Early Bronze Ages to which I might perhaps add the following, R. W. Hutchinson ' Cretan Neolithic Figurines ', *Ipek*, 1938, M. Money-Coutts ' A Stone Bowl and Lid from Byblos ', *Berytus*, 1936.

Miss Kantor gives a very good bibliography for the Middle and Late Bronze Ages to which I venture to add, S. A. Immerwahr, ' Three Mycenaean Vases from Cyprus . . . ', *A.J.A.*, 1945, p. 534; Å. Åkerstroom, ' En mykensk Krukmakverstad ', *Nordisk Familjeboks Manadskronika*, 1939; Dr A. E. Kobev, ' Evidence of Inflection in two Chariot Tablets from Knossos ', *A.J.A.*, 1945, p. 123.

The orthodox statement of Sir Arthur Evans' views has been revised and brought up to date in ' The Archaeology of Crete ' an admirable manual by my friend the late Captain J. D. S. Pendlebury, whose death at the hands of German parachutists in May 1941 was such a loss to British archaeology. Pendlebury made certain corrections and modifications in detail, and in particular reduced the absolute chronology of the revised dates for the Early Dynasties in Egypt, but criticized Reisner's position and has been the only archaeologist to tackle fairly the challenge to the Minoan system of typology and relative chronology championed by Dr Åberg in his *Früheisenzeitliche und Bronzezeitliche Chronologie*, with the exception of Dr Sidney Smith and Professor Childe.

Åberg, while accepting Evans' general typology criticizes the subdivisions M.M. I a and b, M.M. II a and b. He points out that the simpler M.M. III wares often resemble M.M. I a types very closely and betray no knowledge of the stylistic traditions of M.M. I b, M.M. II a and M.M. II b. Certainly it is strange that so many sites should have no objects that can be classified as M.M. II or L.M. II, and it seems more natural to suppose that these were Palace wares current at Knossos (and in the case of M.M. II at Phaistos also) but only represented by sporadic vases elsewhere, than to suppose that so many sites were abandoned during the M.M. II and L.M. II periods. It is natural too, though not inevitable, that we should assume that the period during which the M.M. I a style prevailed over most of the island was comparatively short. Thus the period during which the L.M. I style prevailed over the less luxurious sites (L.M. I and II) occupied less than 200 years (1550–1375).

We may therefore accept Sir Arthur Evans' typology in full and still believe that there was a considerable degree of over-lapping. We may believe, as I do myself, that M.M. I b is typologically older than M.M. I a and earlier than M.M. II a, and yet not exclude the possibility that certain M.M. I b vases were contemporary with M.M. I a types and others with vases of the M.M. II a style. Indeed such overlapping has often been emphasized by Sir Arthur himself. In East Crete there is little of the M.M. I a style so popular in Central Crete; indeed in East Crete the M.M. I b style probably lasted from about 2000 to 1700 B.C. Åberg of course goes farther and maintains that there are only three really distinct periods pre-palatial (E.M. I to M.M. I a) Kamarais (M.M. I b to M.M. III b pre-earthquake) and Late Minoan and that in the Cyclades the Pelos culture also persisted into Middle Cycladic times. Åberg did a real service in emphasizing the fact that the Minoan sub-period styles are not always and everywhere valid all over Crete, but I agree with Pendlebury, whose *Archaeology of Crete* is still the

best general guide to Minoan archaeology, that Åberg went too far. ' However pleasant ', says Pendlebury ' may be the picture of a Sub-Neolithic servant waiting on a M.M. I lady, the fact remains that Knossos is very highly stratified, and that a sufficient number of pure strata exist one above the other to disprove Aberg's case, while by no means disallowing the possibility that the provincial towns may lag behind the great centres of civilization to the extent of missing out a period like M.M. II or L.M. II '. Åberg is an extremely careful and conscientious worker, and his attempts to disprove the stratified deposits of M.M. I b over M.M. I a and of M.M. II b over M.M. II a deserve careful consideration (39). Pendlebury, however, has countered these arguments with a small but complete series of all the Minoan sub-periods (40), and to his list I can add the following pure deposits at Knossos excavated by myself and other members of the British School at Athens.

(i) Late Neolithic deposit West of Northeast Kitchen.
(ii) E.M. III test pit adjoining Royal Road.
(iii) M.M. I a tombs in collapsed cave shelter on Monasteriako Kephali (the classical acropolis of Knossos).
(iv) M.M. I a deposit under M.M. III floor of East Pillar Room.
(v) M.M. III a tombs on Monasteriako Kephali stratified deposit (iii).
(vi) M.M. III b dump on North crest of Monasteriako Kephali.
(vii) L.M. I a dump immediately South of Villa Ariadne, Knossos.
(viii) Late Minoan chamber tomb south of Temple Tomb. First burial L.M. II b, second burial L.M. III a, third burial uncertain date.

These excavations at Knossos since 1935 have not been published in full but the articles on ' Archaeology in Greece ', *J.H.S.*, 1935–46, refer to them.

The justification for Sir Arthur Evans' relative chronology against Åberg is well stated by Dr Smith with reference to the stratigraphy of the Royal Pottery Stores where the three pottery types M.M. I b, II a and II b were found stratified, one separated from the other by a layer of ashes and a floor of clay (41). ' How the layers are to be explained as different phases of one and the same catastrophe at the end of M.M. II has never been elucidated. Floors of a building do not collapse in this way and the description of the floors as " unusually thin ", because they are not so thick as those in another building does not affect the argument from stratification. The attempt to reduce the date of the finds in Egypt to the XVIIIth Dynasty is recognized to be baseless. The inevitable conclusion is that M.M. I b must have been in use in the palace before 1850 B.C. '.

The dating of Tholos B at Platanos must be shortly after 1800 B.C. if our date for Hammurabi is correct since the mature M.M. I a pottery and the Cretan seals in it can hardly be later than 1750 B.C. Yet the Babylonian seal from this tomb cannot be earlier than Hammurabi and might be slightly later.

Some of the deposits I have quoted were not stratified above or under the deposit which should precede or succeed them according to the typology of Sir Arthur Evans, but a pure deposit of any one sub-period is fatal to Åberg's thesis since he would not maintain that M.M. I b was earlier than M.M. I a or M.M. II b than M.M. II a. The spread of the qiuck potter's wheel in M.M. II b is another means for detecting the difference between M.M. II a and b. The slow wheel would not necessarily date a vase to M.M. II a but the use of the quick wheel dates it to M.M. II b or later.

[39] loc. cit. pp. 138–47. [40] *Archaeology of Crete*, Introduction, p. XXXI.
[41] *A.J.A.* XLIX, 1945, pp. 1–25.

It is only at Knossos, of course, that a pure deposit of one sub-period is so damaging to Åberg's theory; elsewhere such as in the Mesara we expect to find pure deposits of M.M. I a continuing late. Reference should be made here to Snijder's claim that the stratigraphic evidence for dating the 'Crocus-Gatherer' fresco to M.M. II b is insufficient, and there is much to be said for assigning it to the M.M. III a period (42).

All this specialized material has to be set in its international context, and certain books which have been invaluable for this purpose, books such as Hall's *Ancient History of the Near East*, Fimmen's *Kretische-Mykenische Kultur*, Dussaud's *Civilizations Egéennes*, and Frankfort's *Studies in Early Pottery of the Near East*, are now beginning to require revision. Bittel's *Praehistorische Forschung in Kleinasien*, still very useful for orientation, requires revision in its chronology.

The English-speaking reader can get most of his international contexts from books such as the *Dawn of European Civilization* and *The Aryans* by Professor V. G. Childe, from Professor Hawkes' *The Prehistoric Foundations of Europe* and from Sir John Myres *Who were the Greeks?* and from the pages of ANTIQUITY.

I propose here to summarize the general position of Aegean chronology as reflected in two recent articles in the *A.J.A.* by Miss (?) Helene Kantor (43) and Dr Saul Weinberg (44). The Cretan evidence though agreeing with a date of about 1800 for Hammurabi makes it difficult to date that king as late as 1712 as suggested by one authority (45) or even 1728 as proposed by Allbright (46). A still more serious objection to a literal acceptance of the Khorsabad king list as it stands is that it gives an impossibly late date to Assuruballit who must overlap the reign of Amenhotep III. Unfortunately the earlier Egyptian dates seem less reliable than when Meyer wrote his 'Aeltere Chronologie' and until the Egyptologists have made up their mind whether Meyer or Neugebauer (47) is nearer the truth the first reliable date would seem to be the heliacal rising of Sothis in the seventh year of Senusert III (48). Another important cross-dating for M.M. II a is provided by the finding of a vase and some sherds of that period at Ras Shamra in a context which the excavator dates to about 1800 B.C. (49).

It is difficult to establish the existence of a direct trade between Greece and Akkad in the third millennium though the evidence for it is accumulating. From Mesopotamia, Homeric Greece obtained its words for gold, and battle-axe. When these borrowings took place is unknown, but it is of interest that the Homeric πέλεκον still preserves the double K sound of the Akkadian *pilakku*. In this connexion Mr Wainwright has made some very interesting remarks on the tin trade (50). The earliest object of pure tin is the bangle discovered by Dr Lamb in the fourth settlement of Thermi on Lesbos (51). Her relative chronology is sound, but her absolute chronology must be reduced now. According to the revision of Aegean dates demanded by the reduced date for Hammurabi,

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[42] *Kretische Kunst.*, pp. 27–30; see also N. Platon in Κρητικὰ χρονικά, vol. I, no. 3, pp. 505–24.

[43] *A.J.A.* II, no. 1 Jan.–March, 1947.

[44] *A.J.A.* II, no. 2 April–June, 1947.

[45] *J.N.E.S.* I.

[46] *B.A.S.O.R.*, no. 77.

[47] *Die Bedeutungslösigkeit der Sothisperiode Acta Orientalia* XVII.

[48] S. Smith, loc. cit.

[49] C. A. Schaeffer, *Ugaritica* I, pp. 55, 56, and figs. 43, 44.

[50] ANTIQUITY, no. 70 (1945), p. 57.

[51] *Excavations at Thermi.*

Thermi IV and the early part of Troy should date from about 2500 to 2200 B.C. rather too early to be drawing tin from the Crisa mine. Of course that does not matter. There may be an undiscovered mine of E.M. I date or the E.M. I workings may have been quarried out entirely in later times. Crisa on the Gulf of Corinth, was probably the nearest tin mine to Lesbos, was in use at least as early as E.M. III times and had become fairly large by the M.M. I period. Thus Anaku Sargon's Tin-land may be Crisa or it might be Laurion in Attica though we cannot prove the latter mines were used so early as the Bronze Age. The fact, however, that Anaku was beyond the Upper Sea and apparently near Kaptara, now generally identified with Crete, suggests that it is somewhere among the Greek Islands or on the Greek mainland (52). Cythera has produced a cuneiform inscription referring to Naram Sin of Eshnunna (53) in the time of the First Dynasty and it is not entirely irrelevant that this island was in classical times the particular home of the cult of Aphrodite.

Yet despite these clear indications of trade between Akkad and Greece during the Early Bronze Age it is still impossible to tie down any sub-period to any particular king of Sumer or Akkad. The so-called ' Sumerian ' head which I excavated at Knossos in 1935 is no help to us. If it be really Sumerian and not a Cretan imitation (as I am inclined to think) it must be about 1,000 years earlier than the objects found with it.

Thus the dating of the neolithic and early bronze Age periods in Greece is still very uncertain and much depends on the dating of the earlier cities of Troy.

The first really important evidence is the lead figurine of the burnt city (Troy II c), which as I pointed out some years ago suggests a correlation with Alishar I b and Cappadocian tablets dated now by Allbright to 1920–1870 B.C., but by Smith somewhat earlier. A new and important piece of evidence is the quadruple spiral silver bead of Troy III type recently discovered by Mallowan at Brak in the upper Euphrates valley. This bead was found in a context which dates it as ' not much before the reign of Naram Sin, or much after the reign of Gimil Sin ', and it is fairly certain it must have been in existence in or about 2100 B.C. (54). This evidence suggests that the end of Troy II was not very long, perhaps a couple of hundred years but not more, before 2,000. On the other hand Trojan synchronisms with Hellas and the Cyclades make it unlikely that Troy V started much later than 1800 B.C. Perhaps Troy II ended about 2200–2100 and Troy V began about 1900–1800. Little excavation (or at least little publication) of remains of the neolithic period was executed for some years after the pioneer work of Tsoundas, Wace and Thompson in Thessaly and the general books of the inter-war period such as those of Miss Hansen and Dr Mylonas reflect this paucity of material (55).

From 1930 to 1939, however, much good work was done. Macedonian Neolithic became a reality thanks to the labours of Heurtley (56), and Mylonas (57), Central Greek Neolithic (58) was illuminated by the work of Kunze and Grundmann, and Peloponnesian Neolithic by the excavations of Drs Walker-Kosmopoulos, Blegen and Weinberg (59). Cypriot Neolithic has been put on a firm footing by the excavations

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[52] Wainwright, loc. cit.

[53] Weidner, *J.H.S.* LIX, pp. 137, 138.

[54] *Iraq*, vol. IX, part I, p. 74.

[55] H. D. Hansen, *Early Civilization in Thessaly*, G. Mylonas, Ἡ Νεολιθικὴ Ἐποχὴ ἐν Ἑλλάδι L.

[56] *Prehistoric Macedonia*.

[57] loc. cit.

[58] *Orchomenos* III and IV.

[59] Weinberg, loc. cit. for summing up and bibliography.

of Dikaios and the Swedish Expedition, and the neolithic cultures of Chios and the Sporades in general are beginning to emerge from the gloom.

In the Cyclades there are indications of a neolithic or sub-neolithic period on Naxos but Stephanos died without publishing more than a preliminary report on his important excavations in that island (60).

The new position of prehistorians towards the Neolithic period in Greece has been well summed up in the recent article in the *A.J.A.* by my friend Dr Saul Weinberg. We can now speak of Early, Middle and Late Neolithic periods, and of sub-neolithic wares contemporary with E.H. I on the mainland and with E.M. I in Crete. The Early Neolithic period is characterized by variegated wares (' rainbow ware '), by black and red monochrome and by pottery with simple linear designs in paint. It is best known by the finds from Corinth and Orchomenos but occurs on other sites in central Greece and the Peloponnese and there is a fine series from Nemea. At present the Early Neolithic pottery of Central and Southern Greece seems rather isolated since most of the Thessalian wares seem to be later but it is likely that the variegated wares described by Tsoundas from his lowest strata at Sesklo belonged to this period (61). The Middle Neolithic period is well represented on the mainland. In Thessaly the ' A ' wares of Tsoundas, Wace and Thompson are the characteristic pottery but the black-burnished wares (Frankfort's ' Danubian ' pottery) came in during this period and was the characteristic pottery at Orchomenos, and in Macedonia from which area it may have spread southwards. To the same period belongs the ' Neolithic Urfirnis ' which is a variation of the monochrome red (A I) but distinguished by the use of a glazed paint as a wash. In the Corinthia the commoner varieties of Thessalian painted and monochrome wares recur together with ' Neolithic Urfirnis ' and with a fine light silver grey ware (62), (distinct from the steel grey I B) which is very widespread in the south and west being found at Malthi in Messenia (63) and at Astakos in Ithaka (64). There are indications that this period which lasted some time, overlapped with Kum Tepe I a and with the earlier part of Troy I in Asia Minor (65).

The Late Neolithic period of Southern Greece was parallel to the second period in Thessaly and to Early Helladic I in some degree as shewn by the stratification at Eutresis (66) and Asine (67), where the excavators found no neolithic stratum but E.M I pottery in plenty. The silver-grey ware continues but the most characteristic Late Neolithic are the matt-painted and polychrome fabrics often referred to as Dhimini wares.

Weinberg uses tubular lugs with ears, anchor ornaments, certain types of bowls, and ' Trojan ' covers to equate Beginning of Thessaly III, Macedonian Bronze Age, and Early Helladic II with the middle of Troy I (the last phrase presumably includes part of Troy I b and part of I c) and the equation seems reasonable (68). Further the bird pins and earliest Pelos ware of the Cyclades seem to be contemporary with Thermi I, Troy I a

[60] *A.J.A.*, vol. LI, no. 2, April–June 1947, pp. 165–82.

[61] Weinberg, loc cit. p. 176 ; Tsoundas, *Eph. Arch.* 1908, pp. 67, 68, p.

[62] Weinberg, loc. cit. p. 174, footnote 71.

[63] Valmin, *Malthi*, p. 272.

[64] Weinberg, loc. cit. quoting Miss Benton.

[65] Blegen, *A.J.A.* XXXIX, 1935, p. 34.

[66] M. Goldman, *Excavations at Eutresis.*

[67] O. Frodin and A. W. Persson, *Asine*, p. 432.

[68] loc. cit. p. 169, foll.

and E.M. I. Pelos ware, however, is a bad type fossil because in some islands it continued well into the Middle Cycladic period as pointed out by Åberg (69). Grave 372 on Syros is important for the dating of the later elements of the Pelos culture. It contained a teapot that recalls M.M. I a types in Crete and Hittite forms from Alaja Höyuk in Asia Minor, a single-handled version of a Trojan beaker such as might be found in Troy II and two bronze pins one with a plain round head, and the other with a head shaped like a grape hyacinth.

Similarly the grave of Hagios Loukas on Syros has a ' Pelos ' jug associated with a Minyan beaker (70). In Crete vases of ' Pelos ' type were discovered in the deposit at Pyrgos near Nirou Khani together with E.M. I pottery (71).

Pendlebury emphasized the fact that the inland distribution of Pyrgos pottery in Crete is against Åberg's theory of a Cycladic colony and speaks rather for a common ancestor (72). This, however, does not invalidate the chronological argument and it is likely that the earliest ' Pelos ware ' overlaps with the E.M. I period ; the same conclusion is suggested by the Cycladic affinities of the primitive tholos at Krasi and certain marble figurines from the early Minoan tombs of the Mesara (73).

It seems evident that Blegen's dates for the earlier levels at Troy must be scaled down in view of his statement that Thermi V ended before the close of Troy I. Weinberg, himself, suggests the equation Kritsana I = middle of Troy I and synchronizes the lowest levels at Kritsana in Macedonia with Thermi III and with E.M. I and II at Eutresis (74).

I am convinced that the evidence suggests a real migration from the Troad into Northern Greece at this time. This Troadic element is dominant in Early Macedonia, competes on equal terms with southern elements in Thessaly, and peters out into a mere trickle but still perceptible on Central Greek sites such as Eutresis.

The Bronze Age really starts in the E.M. I ware as shewn by the evidence from Thermi, Sesklo and Eutresis. The hoard at Hagia Marina in Phokis found with Middle or even Early Neolithic pottery on virgin soil must surely be intrusive (75).

Many authorities now agree in setting the date for the beginning of the middle Bronze Age at or about 2000 B.C. With this dating Dr Weinberg agrees, and he believes that the Middle Helladic and Middle Cycladic periods began at the same time. The type fossil which he quotes is the ' red-cross bowl ' characteristic of early deposits in Troy V but also found in Blegen's B I ware in E.M. III deposits at Kirrha and Orchomenos in Central Greece, at Korakou and Zygouries in the Corinthia and at Malthi in Messenia (76).

Weinberg expresses this as End of Early Helladic = End of Early Minoan = End of Early Cycladic = Early Troy V. Weinberg of course uses the symbol to mean ' overlaps with ', not ' is co-extensive with ', and it is important to remember this. On the whole I think his correlation is sound, but it is always dangerous to assume the vase type, which you know to be a type fossil in one place, is a type fossil elsewhere, and these

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[69] *Bronzezeitliche und Früheisenzeitliche Chronologie.*

[70] Åberg, loc. cit. pp. 100–3.

[71] Åberg, loc. cit. pp. 242–9.

[72] Pendlebury, loc. cit. p. 54, footnote 2.

[73] Xanthoudides, *Vaulted Tombs of the Mesara.* Marinatos, Ἀρχ Δελτ, XII, p. 102.

[74] Weinberg, loc. cit. pp. 168–71 ; H. Goldman, *Excavations at Eutresis.*

[75] Soteriades, *Rev. des Etudes Grecques* XXV, 1912, p. 276.

[76] Weinberg, loc. cit. pp. 179–180.

very ' red-cross ' bowls have been found at Mersin in deposits dating from the Late Hittite Empire (77).

It is a pity that Dr Weinberg almost confines his Asia Minor correlations to the Troad and adjacent islands. Equally important correlations may be made between earlier M.M. I b and M.M. II a shapes and those of Alishar II (78). Here we are confronted by the fact that the sub-divisions of Alishar II are not well defined but we may postulate an overlapping of those two sub-periods in Crete with that of the earlier Hittite Empire. Alishar II in which Cappadocian tablets were also discovered must also overlap, as I pointed out some years ago, with the II c city of Troy (79). Thus if I may borrow Dr Weinberg's phraseology I might say Troy II c = Alishar II = Cappadocian tablets = M.M. I b—M.M. II a though Heaven forbid I should suggest they were co-extensive in time. I think E.H. III must have lasted till 1900 B.C.

I have not referred yet to two important monographs on Aegean art because in the main they deal with aesthetic criticisms of Aegean art rather than with its chronology. The first is ' Die Frühkretischen Siegel ', by Professor Friedrich Matz and the second ' Kretische Kunst ', by G. A. S. Snijder. Matz's conclusions about the ethnic derivations of Minoan culture are highly debatable but his analysis of Minoan motives, his stress on ' torsion ', S-curves and the ' unendliche Rapport ' of the Cretan designs is most valuable. Snijder proposes an ingenious but still more heretical theory that certain peculiarities of Minoan art are due to the fact that its authors were subject to eidetic visions. Snijder, in my opinion, makes out a very good case for his theory but neither of these books affect the chronology to any extent.

Matz, Karo and Snijder have all made some comparisons between Minoan and Helladic art, and Jerzy Pilecki has pointed out the mainland or Helladic character of pyramidal heraldic groups (80).

Miss Kantor, whose article is dedicated to the memory of Captain Pendlebury, starts her survey of Aegean Commerce with the rather sweeping statement ' although Early Minoan culture exerted no influence abroad (81) '. If by ' abroad ' she means outside the Greek archipelago she is probably right and is also probably justified in her statement that the first major expansion of Aegean trade occurred in M.M. II times. She might, however, have mentioned that curious and apparently shortlived extension of Cycladic trade which left its traces in the Middle Cycladic vases discovered in the Balearic Isles (82) and in the Roman cemetery at Marseilles (83). Indeed the bone tubes of Sardinia, the Gironde and Syros in the Cyclades may indicate that western trade had already started in the Early Cycladic period. All the text-books refer to the Melian obsidian, but it is not unlikely that the potter's clay and possibly the silver of Siphnos and the fuller's clay of Kimolos were also factors in this island trade.

In three pages Miss Kantor gives an excellent summary of ' the distribution of Middle Minoan objects in the Orient ' and her footnotes often embody some terse general criticism of value such as the statement on Matz's, ' Die Frühkretischen Siegel ' (84).

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[77] *L.A.A.A.*, vol. XXII, p. 132, a reference I owe to Professor Childe.

[78] Åberg, pp. 211, 212 and figs. 389, 390 (Malla), 391 (Gournia), 392 (Pseira).

[79] *Uruk and Yortan, Iraq.* [80] *Swiatowit*, 1938, pp. 1–61.

[81] *A.J.A.*, vol. LI, Jan.–March 1947, pp. 1–103, ' The Aegean and the Orient in the Second Millennium B.C.'

[82] Reference not to hand.

[83] I noted these on a visit but am not certain if they are published.

[84] loc. cit. p. 20, footnote 31.

' The contribution of Matz to the study of various styles of ornament is very valuable as long as the features he discusses are regarded as objective aspects typical of particular cultural traditions. However, the interpretation of such features as manifestations of certain mysterious psychological predispositions inevitably inherent within certain groups of people is highly controversial and open to doubt '. She expresses herself more trenchantly on a recent article by Dr Schachermeyr (**85**), which she says ' cannot be considered to be on a scientific level as it was completely conditioned by untenable racial and genetic theories that were directly derived, not from scientific evidence, but from the political dogmas prevailing in his country at the time he wrote '.

Schachermeyr, of course, in this and in another article (**86**) is trying to apply to Minoan Art the theories of Clauss (**87**), and Gunther (**88**) in conjunction with Krause's analysis of Keltic art. He did not claim Minoan art to be pure West European but that it was a mixed art with some Western features. The Western features, however, seem a trifle unsubstantial. ' Mother-right ' is not improbable but there is no real evidence of the couvade. As for Schachermeyr's statement that ' there was no humour or eroticism in Minoan art ' (with one exception which he quotes and illustrates) this hardly tells in his favour. Was there no humour or eroticism among the Celts ? Moreover his statement is too sweeping. Minoan frescoes are too fragmentary to support this conclusion and the naturalistic or geometric designs on the pottery are not very expressive mediums either for humour or eroticism.

The rest of the first chapter is concerned with a really excellent account of spiral and maeander decoration in Egypt and the Aegean and how far one may have been influenced by the other. I have only one correction to add here. Rodenwaldt's remark, quoted by Miss Kantor, that ' the details of the Orchomenos ceiling and those of New Kingdom architectural decorations are so different as to preclude the possibility of direct connexions ' must be corrected. The ceiling of Senmut's tomb still has (or had in 1941) a fragment of quadruple spiral with palmette filling like that of the Orchomenos tomb (**89**).

In the second part of her article Miss Kantor discusses L.M. I and II and L.H. I and II vases imported into Egypt and agrees with the view, first advanced by Pendlebury (**90**) and later amplified by Wace and Blegen, that most of these vases originated in the mainland rather than in Crete. It will be remarked that Miss Kantor dubs the vases on her Plate VII Late Bronze I–II Aegean Pottery from Egypt as opposed to the title for Plate VIII Late Helladic I–II, presumably because she regards the first group as either Minoan or Helladic, the second as definitely of mainland origin and the distinction seems not unreasonable. Of the vases on her Plate III, A and D are the two described by Pendlebury as the only certain L.M. vases from Egypt. I am inclined to think that D, E and L on the same plate may also be Minoan but I would not like to guarantee the Cretan origin of the others. It is often very hard to say whether a vase is Minoan or Helladic from the decoration alone. Minoan and Helladic Ephyrean goblets are indistinguishable until you turn the vase upside down when you find that the stem of the mainland goblet is

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[85] *Klio* XXXII, 1940, pp. 339–75.

[86] *Zur Rasse und Kultur im Minoischen Kreta Worter und Sachen*, 1939.

[87] *Rassenkinde Europas*.

[88] *Rasse und Seele*.

[89] Observed by me on the first week of November, 1941 ; I do not remember whether it has been published or not.

[90] loc. cit. pp. 223, 224.

regularly solid, that of the Cretan one partly hollow. One might suggest that the vases had been made in one place and painted in another, but all Ephyrean vases would appear to have been painted before firing.

Miss Kantor discounts some of the Egyptian influence which has been claimed by some scholars as exercising a great influence on the Aegean world. Egyptian objects are rare on Late Helladic I and II sites, and though more common on L.M. I and II sites they are not much better distributed. Mycenaean horses and chariots seem to her, as to me, more reminiscent of Syrian and Hittite than of Egyptian types, nor does she believe much in Egyptian influence on Aegean tomb forms.

She follows this with a good discussion of the Keftiu problem and criticizes Wainwright for accepting ' the textile designs shown in Egyptian renderings of foreigners at face value, ignoring their indigenous character '. In the main she is surely right and the equation Keftiu=Kaptara=Crete is correct, but I think there may be this much truth in Wainwright's interpretation, that Keftiu may have been used to cover not only Minoan Crete, but also, Minoan colonies and trading stations such as that of Trianda on Rhodes, and probably similar ones in Syria and Cilicia.

I must protest, however, that the tholos I opened in December 1939 cannot be later than about 1550 B.C. The material found in and behind the walls in the supplementary excavation in May 1939 (material not seen by Professor Swindler) is entirely M.M. III with the exception of two dark-on-light sherds that might be L.M. I a and a few nondescript fragments that are certainly Middle Minoan but would be hard to date more accurately. In any case my tomb does not of course prove the Cretan origin of tholoi in support of which there are other but still not conclusive arguments. A fair case could be made as Marinatos has pointed out, for the derivation of Cretan and mainland tholoi from the primitive ones in the Cyclades (**92**), but all Aegean tholoi are young compared to those of Arpachiyah in Assyria (**93**).

The third part of Miss Kantor's paper deals with the influence of the Aegean upon the Arts of Egypt and Western Asia in the Late Bronze I and II ages (**94**).

In a very useful discussion of Aegean motives recurring in Egypt, Miss Kantor concludes that the interlocked patterns were probably introduced from Crete but that the contiguous S spirals were certainly Helladic innovations and so probably was the Aa-hotep dagger since the niello technique was characteristic of Mycenaean work and seems to be unparalleled in Minoan Crete.

Many animal motifs in New Kingdom art seem to be affected by Aegean models, but it is hard to say how far these influences came from Crete and how far from the mainland of Greece. Such features include sphinxes of Aegean type (Aa-hotep's dagger (**95**)) the use of the flying gallop and the flying leap in the hunting scenes of the tombs of Puim Re (**96**), Rekhmire and User (**97**) and on certain small objects such as the lid of an ointment box now in the Leiden Museum (**98**).

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[91] *Klio* XXXII, 1939–40, pp. 131–47.

[92] Ἀρχ Δέλτ XII, p. 102.

[93] M. E. L. Mallowan and J. C. Rose, *Prehistoric Assyria*, pp. 25–34, figs. 7–24.

[94] loc. cit.

[95] E. Vernier, *Mémoires de l'Institut Française d'archéologie orientale*, 1907, pl. 24, 2.

[96] Wegner, *Mitteilungen des deutschen Instituts für. Ägyptische Altertumskunde in Kairo* IV, 1933, pp. 79–80.

[97] Davies, *Rekh-mi-Re* II, pl. 48, and *Five Theban Tombs*, pl. 32.

[98] Kantor, loc. cit. pl. XXI C, D.

The chapter ends with a few words on the evidences of Aegean influences on Asia.

The fourth chapter of Miss Kantor's article deals with ' Late Helladic III commerce and its effect upon the art of the Near East ', and makes good use of Furumark's two monumental volumes on Mycenaean pottery (**99**) which has now been subdivided by the Swedish scholar into Myc. I 1550–1500, Myc. II a 1550–1450, Myc. II b 1450–1425, Myc. IIIa1 1425–1400, Myc. IIIa2 early 1400–1375, Myc. IIIa2 late 1375–1300, Myc. IIIb 1300–1230, Myc. III c 1 early 1230–1200, Myc. III c 1 late 1200–1125, Myc. III c 2 1125–1100.

Importation of L.H. III a into Egypt began even in the reign of Amenhotep III and boomed during the reign of Akhenaten ; it fell away somewhat in the succeeding reign but nevertheless stirrup jars of L.H. III b ware were found at Rifeh, Gurob, Balabish and Sedment and also in Nubia. From Palestine and Syria L.H. III a pottery has been reported from Ain Shems, Gezer, Qatna, Tell el Ajjul, Tell Duweir, Tell Abu Hawam, Byblos and Ras Shamra, and L.H. III b ware also from all sites except Qatna. At Atchana the position is obscured slightly by a difference in terminology ; Sir Leonard Woolley's Level O produced Mycenaean which he dated thirteenth century, i.e. L.H. III b. Dr Sidney Smith's level II (dated 1275–1220 approximately) produced some Mycenaean sherds and Level 1220–1190 produced far more.

The sixth city of Troy produced a little L.M. I and II and a quantity of L.H. III a pottery but the pottery from Troy VII a and Tarsus was regularly L.H. III c, often of the Argive ' Granary ' type and the rare sherds from Mersin belonged to the Hellado-Cilician group of L.H. III c pottery.

Signorina Banti in a recent article (**100**) has reviewed the chronology of Minoan pottery in the Mesara and has classified it into Neolithic, Early Minoan (I–III), Middle Minoan, Pre-Palatial, First Palatial and Second Palatial, and Late Minoan (I and III). Signorina Banti bases her classification on the stratification at Phaistos and Hagia Triada. In the Mesara her classification works well enough but I prefer to retain Evans' system for Crete as a whole not because the Knossian series is more typical but because it is the most complete in Crete. Similarly Troadic archaeology must be based on Troy not because that city is the most typical site in the Troad (Troy like Knossos was a centre with many foreign contacts), but simply because the pottery sequence there is most complete. According to Signorina Banti the first palace at Phaistos lasted only from M.M. II b to M.M. III b ; her corrections, however, do not effect the absolute dating.

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[99] (i) *The Mycenaean Pottery, Analysis and Classification* ; (ii) *The Chronology of Mycenaean Pottery.*

[100] Banti, *Annuario della R. Scuola Arch. di Atene*, I–II, New Series, pp. 9–39.

Recent work in the Dodecanese and Cyrenaica

by T. Burton Brown

EX-ENEMY territories, such as the Italian colonies in the Mediterranean, are administered on a care and maintenance basis so long as they remain under temporary allied control during the post-war interim period. It is on this basis that the U.K. undertook the administration of the Dodecanese, Tripolitania and Cyrenaica after British troops had entered those areas and, in one case, until a permanent government was established. The British Administrations set up in those territories have all established Departments responsible for caring for the ancient monuments, and these Departments have been expected to function on the same basis of care and maintenance. If the phrase were interpreted to mean care and maintenance of the antiquities as was done at the time of the Italian Administrations it would involve a very high expenditure, and this has not been possible. But a reasonable amount of work has been done. What has been achieved in two of the territories named is described briefly in the following pages. A short conclusion is added, making some recommendations for the preservation of ancient monuments, since this is a matter, especially in the Mediterranean area, which is not a private local affair but of interest to the whole world.

IN THE DODECANESE

In the island of Rhodes the greater part of wartime damage was suffered by buildings of medieval date in the city of Rhodes itself, and was due mainly to allied naval shelling and bombing. The city walls, described as the 'ramparts of Christendom' when Rhodes, garrisoned by the Order of St. John, constituted a most powerful advanced point against the menace of those days, the Turks, was struck in many places and several churches, and buildings of the Turkish period, were destroyed or damaged. The fine medieval Hospital of the Knights, transformed before the war into a famous museum, was also struck in several places by bombs, but the anti-blast walls constructed by the Italians for its use as an air raid shelter helped to protect it, and the only room totally destroyed was the refectory, though several others were made unusable. Very little damage was done to the Museum exhibits, almost all of which had been stored, though some classical Greek and other pottery was broken, not however beyond repair. A greater loss was experienced in the damage, amounting to virtual destruction, suffered by a large part of the photographic archives.

Apart from the almost complete loss of two or three medieval churches within the walled city of Rhodes, little structural damage was done which cannot be repaired, though the work will be costly and will require a long time to complete, as for example in the case of the medieval walls, where a vast quantity of masonry has collapsed, or is insecure. Comparatively little of such work could be undertaken by the British Military Administration, for financial reasons though, with most welcome assistance from U.N.N.R.A., the weeds and trees of some years unchecked growth were removed from most of the circuit of the walls, and such repairs were executed as were necessary to protect from the weather important buildings, as for example the Houses of France and England. At the same time one or two buildings rendered unsafe during the war, including the Marine Gate, or Gate of S. Catharine, were repaired and brought to a temporary state of security. The major effort in Rhodes on the part of the Antiquities Department was, however, in the direction of reconstructing the Antiquities Service, and to this end

the Hospital of the Knights was brought back into use as a Museum of sculpture, while three rooms therein were furnished with show-cases (constructed mainly from the debris of display cases broken by bomb blast) filled with pottery and minor objects. Another room was filled with medieval war equipment. It was made possible to exhibit a fully representative series of the archaeological material of the Dodecanese, ranging from late Neolithic to medieval days and including all the important classical sculpture found in Rhodes, and some of that from Cos. In the F.E.R.T. Institute of Historical research the library was made available for study, though there was not time to finish checking the catalogue, while in one of the big vaulted rooms of the Institute there was opened an exhibition of part of the rich collection of medieval fabrics, pottery and other handicrafts. This material had been exhibited before the war in a Turkish house incorporated in the Museum, but this was too badly damaged during the war for the limited resources of the Administration to stretch to its repair.

Bombing has its useful side and in Rhodes blast had the effect of revealing under cracked plaster some pleasant medieval frescoes in little mosques, which had originally been churches, but were adapted on the arrival of the Moslems in 1522. (A necessary part of that adaptation was the covering of the paintings). By destroying houses it also revealed the foundations of a wall, made largely of enormous fluted drums of columns, probably from a classical temple, almost at the top of the hill covered by the walled city. Such huge blocks would hardly have been transported far, and it can be assumed that in this are traces of an important local monument of antiquity. Exploration of such finds is not within the scope of ' care and maintenance ', but doubtless it will be examined by the permanent government of the island.

In two of the three other major sites of the island of Rhodes, Lindos and Camiros, there was not very much to be done beyond weeding and cleaning. At Ialysos (Monte Fileremo) however, besides some structural repairs to the 15th century chapel which houses the picture of the Madonna of Fileremo, a treasure of the Order of St. John, the remains of the Byzantine church and of the Hellenistic, and earlier, temple in the same area were freed from the barracks built on them in war time, as well as being weeded and cleaned. The fine Hellenistic fountain nearby, partly dismantled by the Germans for material for anti-blast walls, was reconstructed and its protective railing rebuilt. Little more is required now at Ialysos beyond the routine attention necessary to protect the monuments from wilful and accidental damage.

No very considerable work on ancient monuments in other islands, either through neglect or as a direct result of the war was found immediately necessary and the available resources, aided by most welcome contributions from U.N.N.R.A., were devoted to the repair of the museum building in Cos, built before the war by the Italians but not completed and damaged by shell fire. This building was made ready for the installation of the fine sculpture of the island, which was still housed in its wartime shelter under the Odeon, and several show cases were rcconstructed in Rhodes for the exhibition of some of the pottery. The British Administration, however, left the Dodecanese just before the material could be put on exhibition. Little harm was done in the island of Cos by wartime activlties, apart from damage to the walls of the Castle and looting of some museum exhibits, previously housed therein.

It was difficult, for financial reasons, to replace the formerly numerous custodians at places of interest, but no archaeological areas were closed in default of personal supervision. Redundant entrances to such areas were, however, cancelled, as in the case of the city walls of Rhodes, where numerous unprotected points of entry to the ramparts were walled up, leaving open only the Artillery Gate, by the Castle

forecourt. The desired result, that the ramparts should again form an agreeable place to visit and study the magnificent illustrations of military medieval architecture has been attained, though it was not possible to reconstruct the looted electric lighting system. Admittedly ancient monuments should not be left without protection, supervision and periodic weeding, but this, except for a few places of major importance, would have entailed too great an expenditure for a caretaker government. The co-operation of the local Greek authorities, always interested in, and proud of, their antiquities when they know of them, was of the greatest value under these conditions. In addition, the skill and devotion of the Turkish and Greek members of the Antiquities Department of the British Military Administration, and especially of the Italians, headed by Professor Morricone, the Director of the Italian Antiquities Service of the Dodecanese, made light of what might otherwise have been a difficult piece of work. Moreover, since the Italian Penal Code was still being administered, the legal position with regard to the antiquities was clearly established. Thus when the Greek government took over the administration on the first of April, 1947, the Antiquities Service was a well-equipped concern, with its major activities established.

IN CYRENAICA

Virtually no damage was done to the ancient monuments of Cyrenaica by war-time activities, or by the action of sightseers in war-time. But the Museums of Cyrene and Benghazi had been closed during the war and most of the portable objects, including the library, removed for safe keeping to Tripoli. The statuary was returned by 1946, and a few pieces of sculpture were placed on exhibition in a single gallery at Cyrene. Up to 1947 the main archaeological activity consisted of little more than routine work, such as weeding, underpinning and strengthening of walls, and protection of mosaic and opus sectile floors at the major sites of Cyrene and Tolmeita and the Roman castle of Beni Gdem. In that period there had been a single officer in charge of the antiquities of both Tripolitania and Cyrenaica, but after that date the territories were divided, each with its own antiquities officer. Since then a beginning has been made in the task of exhibiting the more important finds. Permission could not be given to re-open the Benghazi Museum, and consequently the very large store building at Cyrene has instead been transformed into a sculpture museum. This, with lighting directly, and diagonally, from above, is as well lit a place in which to study as one could wish. With the recent opening of the five large galleries here, and with the additional space in the single gallery in the Baths at Cyrene, already in use for exhibition purposes, it has been possible to arrange and put on view conveniently for study all the remarkable and extensive sculpture found in the country. Much of this material has never been published, or exhibited, and includes some important classical Greek and Hellenistic pieces. In addition a representative collection of Greek and Roman pottery, bronzes and other minor objects have been arranged in part of the old Italian Museum, which also houses the archaeological library formed by Professor Oliverio, recently brought back from Tripoli. The very fine photographic library is also at the same place.

Other activity at Cyrene by the Antiquities Department of the British Military Administration has included the normal routine preservation of floors and strengthening of walls. It has also included the work of constructing concrete and masonry supporting walls and beams below parts of the cliff face above the temple area. During the last few years large masses of rock had fallen from this face, and considerable damage might have resulted if the situation had continued unchecked. This work, though largely completed, is unfinished for financial considerations, but already, as a result of it, parts

of the remarkable passages for water cut in the rock at the bottom of this cliff face have been re-opened and cleared of fallen rock and earth, in places twenty feet high. In addition, a few of the more elaborately decorated and well preserved tombs in the northern necropolis were cleared of fallen debris, with a view to their permanent protection. In this operation two funerary statues were found where they had fallen from the top of a tomb. They have now been re-erected in their original positions. Further, some intact fourth century B.C., third century B.C. and first century A.D. graves were discovered.

There are very many places of interest in the country besides Cyrene, and many monuments doubtless remain to be discovered. Amongst the most important sites are Tolmeita, Tocra, Benghazi, Agedabia and Tobruk. At Tolmeita routine weeding and cleaning only was carried out, but at Tocra two intact third century B.C. tombs were opened. The very interesting and important groups of material which were found therein will be published with a description of the contents of the Cyrene tombs. At Benghazi the site of a Hellenistic town was discovered at the landward end of the salt lake east of the modern city. This site is at present in use as a dumping ground for salvage from the battlefields. At Agedabia an important monument is a building with squinches supporting an apse at the end of a barrel vault. This is probably part of an early Moslem structure, and although in a very bad state of repair was not strengthened, as no funds could be made available. Unfortunately, subsequent to its examination in the summer of 1947, more walling has collapsed. At Tobruk there are fairly extensive remains of circuit walls, of rough stones set in a grey mortar which is full of pieces of charcoal, possibly of Byzantine date and built to form a fortified area of rectangular plan, with towers at the corners. Thanks to wartime demolition the circuit can now be followed for much of its length.

The works at Cyrene described above were made possible only by the skill and experience of three Italians, all of whom had worked at Cyrene before the war, and had volunteered to return there from Tripoli in 1945.

CONCLUSION

From a short experience of the requirements of the care and maintenance of ancient monuments in territories under temporary British Administration, three conclusions have been drawn. The first, and most obvious, is connected with money. Clearly, after a war, either because of destruction from explosives or through unavoidable neglect of monuments, capital expenditure cannot always be avoided. If care of antiquities is run on a strict yearly budget basis, expensive damage may develop before funds are available for its prevention. And consequently requests for special grants of money for individual works should not be rejected freely. The second conclusion relates to the establishment and legal basis of the Antiquities Department of the Administration concerned. It is possible that the organisation of the care and maintenance of antiquities involves more factors than can satisfactorily be dealt with, except on the basis of a fully detailed Antiquities Law. The archaeologist who works for a British Military, or any other sort of Administration naturally owes allegiance to that administration. But he also owes allegiance to his science, and without common ground between the two, as is afforded by such a Law, it may be made very difficult for him to carry out his work as it should be done. The third conclusion is that it is essential to have some trained and experienced specialists working in the framework of a properly constituted organisation. There are always ways of getting materials, but there is no way round the lack of efficient executives. And without them tragic damage may result at the hands of inefficient local personnel.

Open Arable Fields at Portland and elsewhere

by Charles D. Drew

EXAMPLES of Open Arable Fields in cultivation at the present time are so rare (1) that it is strange that those in the Isle of Portland in Dorset have been overlooked by every writer on this subject. This omission is probably due to the isolation of Portland, thrust, as it is, far out into the waters of the English Channel. Its isolation, also, is probably one reason why these open fields have survived, for Portland is a district of survivals. The Court Leet of its Royal Manor still exercises real authority, and Portlanders are tenacious of their ancient rights and customs. They tolerate but are not over-fond of 'foreigners' from the mainland and they sturdily resent outside interference and direction.

The area of the old manorial plan is considerably defaced, for, especially during the last century, much of the surface of Portland has been transformed. The great citadel of the Verne, guarding and looking down on the Harbour and Naval Base, covers much of what was the common pasture of the manor. Greater still are the results of quarrying for the famous Portland Stone. This quarrying naturally involves the removal of the overburdon, and with it great areas of the open fields have vanished for ever. This necessary process—for Portland lives by its stone—is now carried out by mechanical excavators, with ever-increasing speed, but quite a wide extent of open fields still remains in being. One can still see several adjacent furlongs (or groups of strips) with their component strips ('lawns' is the local term) (2) in cultivation. In some cases the strips have been thrown together to form larger areas, while in others the strips have been fenced with dry stone walling (PLATE II, A). In the neighbourhood of some of the larger quarries—Broadcroft, for example—large areas have gone out of cultivation, though the strips can still be traced clearly, and are sadly awaiting the approaching excavators.

One feature, which is absent from other surviving examples of open arable fields, but which exists here, is the dividing balk of unploughed turf between the lawns (3). (PLATES I and II, B).

This is important, for the very existence of such balks has been denied in no uncertain terms. They are first mentioned by Seebohm (4), who took as his example of open fields those in the township of Hitchin, Herts., where, when he wrote in 1883, the remains of the open fields were still in existence. His words regarding balks are as

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[1] C. S. and C. S. Orwin, *The Open Fields*, p. 45, give only three—Eakring and Laxton in Nottinghamshire, and the parishes of the Isle of Axholme in Lincolnshire, to which they add Elmstone Hardwicke in Gloucestershire, which was enclosed as recently as 1914.

[2] Lord Ilchester kindly informs me that at Abbotsbury in Dorset the terraces formed by the fine series of linchets on the side of St. Catherine's Hill are also called 'the Lawns'. So the term was not confined to Portland.

[3] In Portland the local term for a balk is 'lawnshed'.

Mr W. J. Hooper kindly tells me that when a boy he was told by an old carter that he had ploughed up the balks between the strips in what was Fordington Great Field, the land which Mr Hooper now farms. This ploughing up would have been in the 1860's.

[4] Frederic Seebohm, *The English Village Community*, 4th edn., p. 2.

follows :—' These strips, common to open fields all over England, were separated from each other not by hedges but by green balks of unploughed turf . . . '. The phrase ' all over England ' refers to the strips and not to the balks, and Seebohm is only claiming that balks existed at Hitchin, and surely he knew, for his study must have made him familiar with every feature there. But later writers assumed without question that the balk was the invariable boundary between strips wherever the latter existed in England.

This view was not challenged until 1938, when Dr and Mrs C. S. Orwin, in an otherwise able and practical review of open fields, discussed *inter alia* the question of balks. After a reasoned survey of the facts known to them, they came to the conclusion that deep furrows and not balks formed the boundary between strips, and that ' these balks were the creation of Seebohm and had no existence in reality ' (5).

This conclusion was not allowed to pass without a challenge. Dr E. Cecil Curwen examined each of the Orwins' reasons for their contention and gave his objections to them in detail (6). He did not, however, press fully home his counter-attack on the denial of the balk. He said ' We may also be prepared to accept the arguments that in the open field system such balks were not normally present '. But neither he nor the Orwins were aware of the evidence supplied by these Portland fields, where the balk exists as the boundary of every strip, except where strips have been thrown together or fenced with stone walls.

So the balk is re-established. But one must not be dogmatic about it. It is evident that the Orwins' contention was far too sweeping, but I readily admit that they are perfectly correct as far as concerns the heavy clay areas of the Midlands, which they know so well. The problem there was to get rid of a superfluity of water, and this was solved by the deep furrows which there formed the boundaries of the strips. But this problem does not arise in the lighter and drier soils of the limestone and chalk country (of which I can claim to have some knowledge) where the balk was widely, and perhaps always, used. At bottom, the reason is one of Geology.

Strangely enough, the balks are being preserved at Portland at the present moment, even when many lawns which had fallen out of cultivation have been reploughed again by order of the County War Agricultural Committee. One might have thought that, with the urgent need for more arable, these hindrances to the complete cultivation of a field would have been swept away. Their preservation illustrates how much the present is bound up with the past. For in Portland, until 1926, the system of Gavelkind (7) governed the inheritance of land when the owner died intestate, and the land went, not to the eldest son alone, but to all sons equally. The land itself was not divided, but only the rights therein, so that it is still possible for a Portlander to own, say, an undivided third part of an undivided fifth part of a lawn. These complications of ownership (to

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[5] C. S. and C. S. Orwin, *op. cit.*, pp. 43–8 and 321, 322. Professor R. F. Treharne, *Common Errors in History*, 2nd Series, Historical Association (1947), p. 6, is even more emphatic. He says :—' The dividing baulk between strips . . . must be discarded as the invention of prejudiced ignorance ; it never had any basis in fact '. This can only refer to Frederic Seebohm, and I cannot allow it to pass without protest. His pioneer work on Open Fields, to which all students owe so much, deserves more generous reference.

[6] E. Cecil Curwen, *The Plough and the Origin of Strip Linchets*, ANTIQUITY, XIII, pp. 45 ff.

[7] The presence of Gavelkind in Portland is interesting. I leave to others more qualified than myself to judge if it may be an indication that Jutish coastal settlements extended farther west than has been hitherto recognized, i.e., the Hampshire Avon. It is perhaps significant that Gavelkind was ' the custom of the town ' at Wareham in 1316. (Assize Roll, No. 1370 m.14, 8 Sept., 10 Edw. II).

speak rather loosely, for many holdings are held on Crown lease) make it essential that the exact area of each lawn should be clearly recognized on the ground, and this necessity is increased by the high value of land at Portland, due to the potential value of the underlying stone. PLATE I shows such re-ploughed lawns.

The same plate illustrates another feature. The lawns cross the head of a small valley at right angles to it. For much of their length the balks are only a few inches above the level of the ploughed surface. But in places, especially in the valley bottom, each balk gradually becomes a linchet, rising in some cases to as much as two feet at its greatest height. As the lawns have not yet become level terraces, it is evident that the growth of the linchets is still in progress. So one does not now need to go to Belgium, as Dr Curwen did, to find a ' living ' example ; Portland affords an even better one.

Now the Orwins use their denial of the existence of the balk to sustain another assertion—that strip cultivation ' has no connexion whatever ' with linchets (8). They rightly say that without a balk no linchet could be formed. This is well illustrated in PLATE II, B, where, as the strips on the left run downhill, no linchet has formed, except at the bottom of the strips, which is just outside the photograph. But, and I think this proves that strips and linchets have a very intimate connexion, on the right of the same plate, where the strips run at right angles to the slope, each balk has become a very pronounced linchet. At Portland, at any rate, the strips fit the linchets, as Seebohm would have said, as a snail into its shell.

The Orwins have observed that some linchets occur in places where it is unlikely or even impossible that they could have had any connexion with an open field. This may well be so ; I agree that there may be some ' pseudo-linchets ', formed by some other agency than the plough. But others, whose position at first sight appears to be difficult to explain, can be understood if it is possible to reconstruct the plan of the now vanished open fields, of which they are one of the new remaining vestiges. If one can obtain a pre-enclosure estate map, it is an interesting archaeological exercise to plot on the same 6-inch Ordnance Map sheet, both the furlongs, strips and linchets shown therein and also the existing linchets on the ground. In two cases in Dorset, Godmanstone and Sutton Waldron, where I have done this, they fit together as neatly as at Portland.

So I maintain that Seebohm is in the main correct in his statements regarding strips, linchets and balks. If any remain unconvinced by the evidence I have produced, I beg them to go to Portland and see for themselves.

The open fields of Portland demand competent study and adequate publication, and it is satisfactory to be able to state that arrangements for this to be undertaken have been made by the History Department of the University College of Southampton.

[8] C. S. and C. S. Orwin, *op. cit.*, pp. 321, 322.

Nechtanesmere

by F. T. Wainwright

IN 685 Ecgfrith, King of Northumbria, led an expedition against the Picts. North of the Tay, at Dunnichen near Forfar, he was slain and his army was destroyed by the forces of Brude mac Beli King of the Picts. The battle was fought near a loch, Symeon of Durham's ' Nechtanesmere ', which scholars of the 19th century believed to have existed in the mosslands of Dunnichen. The site of the battle is not known, the loch itself has now disappeared, and the accounts of local historians and others, neither properly checked nor fully assimilated by national historians, are not all easily accessible. Floods which followed the winter of 1946–47 temporarily restored at least part of the old loch or mere, and in the summer of 1947 the flood-marks in the fields could still be seen. It seemed worth while to try to indicate on a map the extent of the vanished loch and this, with the aid of photography and a field-survey, has been possible. It seemed worth while also to sift the meagre scraps of evidence provided by chroniclers and, often unintentionally, by local writers of the last century, for these scattered scraps deserve to be brought together and re-examined.

Normally one would not be surprised to find that the details of a battle, fought over twelve centuries ago in a remote part of Britain, are blurred and badly recorded. But the Battle of Nechtanesmere, as it is usually called, is mentioned by several independent and contemporary writers, and it is an unfortunate chance that not one of them gives a full account of it. Adamnan (biographer of St. Columba), the unknown monk of Lindisfarne (biographer of St. Cuthbert), Eddi (biographer of St. Wilfrid), and Aldfrith (learned half-brother and successor of Ecgfrith) could all have written detailed and trustworthy accounts of the battle, and all had reason enough for doing so. Bede himself, though only a child of twelve in 685, would also be familiar with stories of the battle and could have answered most of our questions. As it is, however, the battle is first named in the Irish chronicles, the English name for it does not appear until Symeon of Durham recorded it in the 12th century, and we are left to guess at the purpose of the expedition, at the route followed, at the size of the armies, etc.

Adamnan, writing about six years after Ecgfrith's death, mentions the *bellum Ecfridi* (1), but his single reference is introduced simply to provide a well-known incident by which other events can be dated. He tells us nothing about the battle itself ; we learn only that there was a battle and that it was generally regarded in the late 7th century as one of the outstanding events of the age. As Abbot of Iona, where the bodies of Ecgfrith (2) and Brude (3) were brought to rest, and as the close friend of Aldfrith,

[1] *Adamnan's Life of St. Columba*, p. 186, ed. William Reeves, *Irish Archaeological and Celtic Society*, Dublin, 1857.

[2] Symeon of Durham has preserved the information that Ecgfrith was buried in Iona (*Symeonis Monachi Opera Omnia*, ed. Thomas Arnold, vol. I, p. 32, Rolls Series, 1882).

[3] An Irish Life of Adamnan preserves a curious story which suggests that Adamnan was on terms of friendship with Brude mac Beli as well as with Aldfrith : the body of Brude was carried to Iona, and Adamnan, being grieved and sorrowful at his death, set about restoring him to life—with conspicuous success until he was persuaded to desist by a certain religious man who argued that other priests, lacking the power to raise the dead, would feel their deficiency to be a disgrace. The text and a translation of this story from Brussels MS. 5101–4 is provided by W. F. Skene, *Chronicles of the Picts and Scots* (1867), pp. 408–9. See also *Betha Adamnáin*, pp. 16–17, edited by R. I. Best (*Anecdota from Irish Manuscripts*, vol. II, 1908) and translated by M. Joynt (*Celtic*

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whom he knew before his accession and whom he visited in Northumbria after 685, Adamnan was in an excellent position to know both the Northumbrian and the Pictish versions of the battle. Unfortunately he is silent. Some ten or fifteen years later (i.e. between 699 and 705) a monk of Lindisfarne wrote a life of St. Cuthbert (4). From him we learn the day and time of the battle—it was fought on a Saturday, about the ninth hour (5). But the unknown author of *Vita Sancti Cuthberti* was not interested in Ecgfrith and Nechtanesmere ; he was interested in St. Cuthbert who happened to have prophesied Ecgfrith's death and who, being at Carlisle on the fateful Saturday, happened to have a premonition of the disaster that had overtaken the Northumbrian forces (6). Both Cuthbert and his biographer would hear full accounts of the battle, but Cuthbert was reluctant to reveal even the details of his vision and from neither has anything like an official report survived. Bede's *Vita Sancti Cuthberti*, modelled upon that of the unknown monk, repeats the stories of Cuthbert's prophecy and premonition ; though less vivid than its naïve original it is superior in its lucidity and in its author's control of the Latin language. Bede fills in many details, and with regard to the Battle of Nechtanesmere he adds the interesting information that Ecgfrith's bodyguard was slain around him (7)—one more fact known about the battle and one that can be trusted. It was fitting that a king's personal followers should die with their lord, but we need not dismiss this additional fact as mere literary invention. Bede and his contemporaries had grown up with stories of the disastrous northern campaign and it is highly unlikely that he was mistaken about a point of such real importance to men of his own time. He could have given us many more details of this kind, had he wished.

Indeed when he came to write his *Historia Ecclesiastica Gentis Anglorum* (completed in 731, i.e. ten or a dozen years after his prose life of St. Cuthbert was written) Bede did give a few more details (8). He tells us that Ecgfrith's campaign was undertaken against the advice of friends and especially against the advice of Cuthbert who had recently been consecrated bishop (9). He tells us the date of the battle—it was fought on 20 May 685

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Review, vol. v, 1908–9, pp. 102–3). See also A. O. Anderson (*Early Sources of Scottish History*, 1922) who gives references (vol. I, p. lxxiii) and a translation (ibid, p. 201). Brude died in 693 and must then have been a very old man (ibid, p. 193).

[4] *Vita Sancti Cuthberti Auctore Anonymo*, ed. Bertram Colgrave in *Two Lives of Saint Cuthbert*, Cambridge, 1940.

[5] i.e. in the afternoon, probably about three o'clock.

[6] An interesting case of second-sight, witnessed according to the monk of Lindisfarne by several persons who were still alive when he was writing. Cuthbert had gone to visit Ecgfrith's queen at Carlisle where she was waiting for news of the Northumbrian expedition. On the Saturday at the ninth hour a tour of the local Roman remains, under the leadership of Waga the reeve, was interrupted by Cuthbert who, displaying signs of distress and raising his eyes to heaven, exclaimed : ' Oh, Oh, Oh, I believe the war is ended, and the verdict has been given against our warriors '. He evaded urgent requests for an explanation but ' after a few days they heard that it had been reported far and wide that there had been a deplorable and lamentable battle on the very day and at the very hour in which it was revealed to him ' (ibid, p. 122).

[7] *Vita Sancti Cuthberti Auctore Beda*, ed. Bertram Colgrave, op. cit. p. 248. Bede's prose life of St. Cuthbert, here quoted, was written in or a little before 721. Bede had earlier written a metrical life of St. Cuthbert (see *Bedas metrische Vita sancti Cuthberti*, ed. Werner Jaager, Leipzig, 1935) in which (Jaager, pp. 105–6) Cuthbert's vision is described but with fewer details of time and place.

[8] *Historia Ecclesiastica Gentis Anglorum*, Lib. IV, Cap. 24, *Bedae Opera Historica*, ed. C. Plummer (Oxford 1896), vol. I, pp. 266–7.

[9] On Easter Day, 26 March, 685.

(*die* XIII. *Kalendarum Iuniarum*), in the fifteenth year of Ecgfrith's reign and in the fortieth year of his life. And he explains that the disastrous struggle was really a rout which followed the success of an ancient strategem : Ecgfrith ' was drawn on, by the feigned flight of the enemy, into the narrows of inaccessible mountains and was slain with most of the forces he had taken with him '. We are not told either the name of the battle or where it was fought, but this is the fullest account that has survived and, written by a careful historian with access to the facts, it can be trusted. Despite the fact that Ecgfrith had granted land for the foundation of Bede's own monastery Bede clearly deplores his expedition against the Picts and regards his fate as divine retribution for the merciless ravaging of Ireland, also against advice, in 684. From Bede, at any rate, we get the impression that Ecgfrith was an aggressor, if not a wanton aggressor.

Eddi, or Eddius Stephanus, wrote his *Vita Wilfridi Episcopi* within a generation of Nechtanesmere and about ten years before Bede wrote his prose life of St. Cuthbert. He gives an account of the Pictish revolt which Ecgfrith crushed early in his reign, but he ignores Nechtanesmere except for the oblique comment that nothing went well with the king after his quarrel with Wilfrid (**10**). Eddi might have told us more. Eadmer, writing over four hundred years after the battle, could have had no such personal knowledge of either the saint or his royal enemy, but he agrees that fate was unkind to Ecgfrith. Indeed he tells us that Wilfrid, while celebrating mass in Sussex, had a vision in which he saw Ecgfrith slain and his soul carried off to Hell by two evil spirits (**11**). It is no doubt the kind of vision that Wilfrid would have chosen for himself, but as an unpleasant detail of the battle we must reject it as late, improbable, and of doubtful origin (**12**).

The death of Ecgfrith is recorded *sub anno* 685 in MSS. A, B and C of the *Anglo-Saxon Chronicle* (**13**). In MS. E there is a fuller account ; the scribe follows Bede in giving the exact date but adds an explanatory note that the king was slain *be norþan sæ*. There is no need to seek a source other than Bede behind this annal, although in point of fact many details not preserved by Bede must have been current in the north for a long time after the battle. For example, among notes to some Northumbrian genealogical material which now forms part of the composite work attributed to Nennius, is preserved the British name of the battle : *Gueith lin(n) garan*, ' the battle of the pool of Garan (**14**) '. And the English name—*Nechtanesmere, quod est stagnum Nechtani* (' the swamp or lake of Nechtan ')—is recorded by Symeon of Durham (**15**) who was no doubt following an earlier Northumbrian account which contained not only the name of the battle but also the statement that Ecgfrith was buried at Iona.

The name that means most to a modern reader is the Battle of Dunnichen, and this is found in Irish chronicles. Irish annalists were familiar with the work of Bede and

[10] *Vita Wilfridi Episcopi auctore Eddio Stephano*, cap. XIX, ed. James Raine in *The Historians of the Church of York*, vol. I (Rolls Series, 1879), and *The Life of Bishop Wilfrid by Eddius Stephanus*, ed. Bertram Colgrave (Cambridge 1927), p. 40.

[11] *Vita Wilfridi Episcopi auctore Eadmero*, cap. XLIII, ed. James Raine, op. cit.

[12] Eadmer himself apparently does not feel altogether easy in his mind about this story for he writes (cap. LVII) : *Illud tamen quod dixi de damnatione regis Ecfridi fateor nusquam legi ; sed tot talesque viri id ita se habuisse confirmant, ut eis nolle credere magnae impudentiae esse crediderim.*

[13] *The Anglo-Saxon Chronicle*, ed. Benjamin Thorpe, Rolls Series, 1861.

[14] *Historia Britonum* in *Monumenta Historica Britannica* (London, 1848), p. 74. The passage may be translated thus : ' This Ecgfrith is he who made war against his cousin (*fratruelem*), Brude King of the Picts. There he perished with all the best of his army, and the Picts with their king were the victors. The Northumbrians have never managed to exact tribute from the Picts since the time of that battle [which] is called *Gueith Linn Garan* '.

[15] *Historia Dunelmensis Ecclesiae*, cap. IX (*Symeonis Monachi Opera Omnia*, loc. cit.).

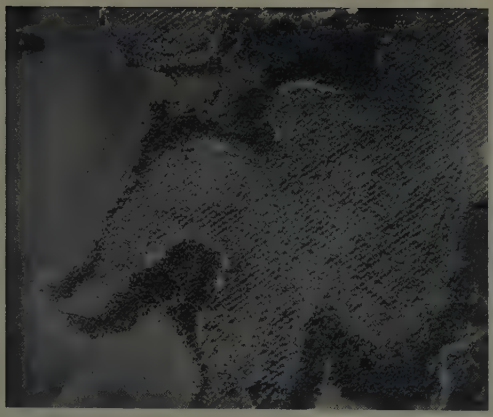
PLATE I

PORTLAND: BRANSCOMBE FURLONG, STRIPS AND BALKS

facing p. 84

A. PORTLAND: STRIPS FENCED WITH DRY STONE WALLS

B. PORTLAND: STRIPS OF ONE FURLONG BUTTING AGAINST THOSE OF ANOTHER



(*Top left*) SOUTH WEST JOHORE, MALAYA. OLD BEACHES BEING SMOTHERED BY ADVANCING MANGROVE. May 1946. Scale, 1.7,000. R.A.F. Crown Copyright Reserved

(*Top right*) SINGORA (SONGKHLA), SIAMESE MALAYA, 7° 13′ N, 100° 35′ E. DARK AND LIGHT LINES OF BEACH 'BUILDUP'. The ringed area, partially cloud obscured, is a rocky outcrop which was formerly an island. February 1945. Scale, 1.52,000

(*Below*) PT. COOK, VICTORIA, AUSTRALIA. May 1942. Scale, 1.11,000

other scholars in Britain, but their links with Iona provided a second channel for information about events in the far north, and it is highly probable that the name of the battle reached Ireland through Iona. There apparently once existed a chronicle compiled in Iona, and from this rather than from an Anglo-Saxon source many notices of events in northern Britain, including the following notices of the Battle of Nechtanesmere, found their way into the major Irish chronicles as we have them to-day. The *Annals of Ulster*, a late compilation but based upon contemporary material from the late 7th century onwards, have the following entry *s.a.* 685 (16) :

> *Bellum duin Nechtain uicisimo die mensis Maii sabbati die factum est, in quo Etfrith mac Ossu rex Saxonum, xv anno regni sui consummata magna cum caterua militum suorum interfectus est . . .*

From the same underlying sources there appears in the so-called *Annals of Tigernach* (17) a similar entry which may be translated thus :

> The Battle of Dunnichen (*Cath Duin Nechtain*) was fought on the twentieth day of the month of May, a Saturday, and there, having reigned for fifteen years, Ecgfrith son of Oswiu and King of the Saxons was slain, together with a great number of his warriors, by Brude son of Bile the king of Fortriu.

A curious compilation, which survives only in a late MS. and which is generally known as the *Three Fragments of Irish Annals* (18), contains the brief note :

> *Cat*[*h*] *Duin Neac*[*h*]*tain itt*[*orchair*] *mac Ossu. Bruite m*[*a*]*c Bile victor fuit* (19)

which may be translated :

> The Battle of Dunnichen in which fell the son of Oswiu. Brude, Bile's son, was the victor.

This Irish collection of annals has never been subjected to a critical analysis. Though now embedded in legend and overlaid with literary embellishment its ultimate sources undoubtedly include much genuine historical material (20). At some undetermined point in the development of these annals a compiler, confusing Ecgfrith and Aldfrith, incorporated under a note on Aldfrith's death some verses on the Battle of Nechtanesmere and attributed them to ' Riaguil of Bangor '. These verses are difficult to translate (21) and they do not add much to our knowledge of the battle, but they may represent an early comment on what was one of the most important events in the history of the north.

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[16] *Annals of Ulster*, vol. I, pp. 134–6 (ed. W. M. Hennessy, Dublin, 1887).

[17] ed. Whitley Stokes, *Revue Celtique*, vol. XVII (1896), p. 209.

[18] Bibliothèque Royale, MS. 5301–5320. This compilation was edited by John O'Donovan as *Annals of Ireland, Three Fragments* (*Irish Archaeological and Celtic Society*, Dublin, 1860).

[19] MS. fol. 12*b*. O'Donovan (op. cit. p. 88) silently extends the abbreviation *itt̄* to *ittir* (instead of to *ittorchair* as above) and makes other changes from the text of the MS. If *ittir* were accepted, the translation would run : ' The Battle of Dunnichen between the son of Oswiu [and] Brude, Bile's son, [who] was the victor '. But the extension and the translation given above are preferable.

[20] See, for example, F. T. Wainwright, ' Ingimund's Invasion ' (*English Historical Review*, vol. LXIII).

[21] Translations by John O'Donovan (op. cit. p. 111) and W. F. Skene (*Chronicles of the Picts and Scots*, p. 402, and *Celtic Scotland*, vol. I, pp. 266–7) are unsatisfactory. A. O. Anderson gives a translation (*Early Sources of Scottish History*, vol. I, pp. 194–5) but for some time he has been engaged upon the preparation of a revised translation and a discussion of the complex problems raised by these verses. Dr Anderson's generous and patient explanations of these and related problems have saved me from many errors into which I should otherwise have fallen.

Other references need not be discussed (22). Chroniclers like Florence of Worcester, Henry of Huntingdon and William of Malmesbury for the most part follow Bede—sometimes adopting his words and phrases—and other comments in verse and prose only emphasize that the battle made an impression on writers who were far removed from it in time.

What do we learn from an examination of the primary sources ? We learn that Ecgfrith, King of the Northumbrians, led an expedition against Brude mac Beli and the Picts, that his army was routed and he himself slain in a battle fought during the afternoon of Saturday 20 May 685 (23), and that the Picts had manoeuvred the Northumbrians into difficult country by feigning flight. The actual clash of forces would be a mêlée, for the Northumbrian ranks would be broken by their pursuit of the Picts. Amid the general confusion, however, at least one group of warriors made a last stand around their king. Ecgfrith's body was taken to Iona, and we have only Eadmer's word for the fate of his soul. The defeat was decisive in the extreme : most of the invading Northumbrians were killed, and the few who survived no doubt saved themselves by flight (24). The size of the Northumbrian expedition is not known and its purpose is not stated. It is unlikely that Ecgfrith had exercized any real control north of the Tay but earlier in his reign he had successfully crushed a revolt of the Picts, who ' were threatening to cast off the yoke of slavery ' (25), and this implies at least a Northumbrian claim to lordship over the Picts. The expedition of 685, however, was undertaken against the advice of Cuthbert and other responsible men in Northumbria, and it is represented by English writers as an unjustifiable act of aggression. The battle was fought near a pool or loch. It was known to the Angles as the Battle of *Nechtanesmere*, to the Britons as *Gueith Linn Garan*, and it appears in Irish sources as *Cath Duin Nechtain*.

Dun Ne[*a*]*chtain*, ' the fortress of Nechtan ', is the modern Dunnichen (26), a village which lies about three miles southeast of Forfar and about nine miles from the sea at Arbroath. This identification is due to George Chalmers (27). His work is often criticized by later scholars such as W. F. Skene but in this instance full credit must certainly be given to him. He is followed by a host of writers : James Knox (28), Andrew Jervise (29), John Stuart (30), William Reeves (31), James Taylor (32), John

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[22] Other references are given by A. O. Anderson in *Scottish Annals from English Chroniclers* (1908), pp. 42–3, and *Early Sources of Scottish History* (1922), vol. I, pp. 192–5.

[23] The twentieth day of May was a Saturday in 685.

[24] Cf. William of Malmesbury's statement that '*pauci fuga lapsi rem domi nuntiarunt*' (*Willelmi Malmesbiriensis Monachi De Gestis Regum Anglorum Libri Quinque*, ed. William Stubbs, vol. I, p. 57).

[25] *Vita Wilfridi Episcopi auctore Eddio Stephano*, cit. sup., cap. XIX.

[26] Note the following selected spellings of the place-name Dunnichen (dates in some cases are approximate) : 1178 *Duneicht*[*in*] ; 1178 1210 1214 1218 1220 *Dunectin* ; 1182 1190 1200 1212 1215 1248 1304 1315 1322 1351 *Dun*[*n*]*echtyn* ; 1215 *Dunechtin* ; 1457 *Dunnychtin* ; 1483 1486 1509 *Dunnychtyn* ; 1592 *Dunnichtin* ; 1329 1461 1503 1517 1536 *Dun*[*n*]*echyn* ; 1517 *Dunnechin* ; 1528 *Dunnychyn, Dunnychen*. These forms are from the *Liber S. Thome de Aberbrothoc*, ed. Cosmo Innes and Patrick Chalmers (*Bannatyne Club*, 1848–56).

[27] George Chalmers, *Caledonia* (1807–24), vol. I, pp. 210, 254–5.

[28] *The Topography of the Basin of the Tay* (Edinburgh, 1831), p. 118.

[29] *Proceedings of the Society of Antiquaries of Scotland*, vol. II (1854–7), p. 189 ; *Memorials of Angus and Mearns* (Edinburgh, 1861), p. 21.

[30] *Sculptured Stones of Scotland*, vol. I (1856), p. 29.

[31] *Adamnan's Life of St. Columba* (1857), cit. sup., pp. 186–7.

[32] *The Pictorial History of Scotland* (1859), vol. I, p. 26.

O'Donovan (33), E. W. Robertson (34), J. Hill Burton (35), W. F. Skene (36), W. Marshall (37), J. Romilly Allen (38), A. J. Warden (39), James Gammack (40), W. M. Hennessy (41), Charles Plummer (42), Thomas Thomson (43), P. Hume Brown (44), D. Christison (45), T. Hodgkin (46), Charles Oman (47), H. E. Maxwell (48), J. C. Watt (49), Archibald B. Scott (50), F. M. Stenton (51), etc. This list is not intended as a complete list of references, which of course it is not, but as an illustration of the wide acceptance of Chalmers's identification. It includes writers well known and writers unknown, writers of very different levels of ability and reputation. There are historians of the highest rank, archaeologists, skilled antiquaries, unskilled antiquaries, and authors whose work properly belongs to the realms of fantasy. Some would be shocked to find themselves in such strange company and some should be amazed to find themselves in such distinguished company. None add anything by way of identification to what Chalmers wrote in 1807. Those who, perhaps unwisely, give details do not give new or important details; they follow Chalmers in his evidence (52), his errors and his guesses (53), and sometimes they manage to distort even these. Few refer to Chalmers—some do not give references at all—and to-day the historian may well find that Chalmers is hidden from view by the names of men like Jervise and Skene. Critics have not always judged Chalmers's achievement against the background of his difficulties, and his work

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[33] *Annals of Ireland, Three Fragments* (1860), cit. sup., p. 89.

[34] *Scotland under her Early Kings* (Edinburgh, 1862), vol. I, p. 12.

[35] *The History of Scotland* (1867–70), vol. I, p. 312.

[36] *Chronicles of the Picts and Scots* (Edinburgh, 1867), pp. cxix, 402; *Celtic Scotland* (Edinburgh, 1876–80, 2nd ed. 1886–90), vol. I, p. 266.

[37] *Historic Scenes in Forfarshire* (Edinburgh, 1875), p. 118.

[38] *The Early Christian Monuments of Scotland* (Edinburgh, 1903), Part III, p. 207; *Proceedings of the Society of Antiquaries of Scotland*, vol. XVI (1881–82), pp. 103–4.

[39] *Angus or Forfarshire* (Dundee, 1880–85), vol. I, p. 25, vol. III, p. 189.

[40] *Memorials of Angus and Mearns* (1885), vol. I, p. 24. This work is merely the second edition of Andrew Jervise's work (1861) of the same title. Gammack reprints Jervise's account but a misprint introduces an error in the date of the battle.

[41] *Annals of Ulster*, cit. sup., vol. I, p. 135.

[42] *Bedae Opera Historica*, cit. sup., vol. II, p. 261.

[43] *A History of the Scottish People* (1896), vol. I, p. 51.

[44] *History of Scotland* (1899–1902), vol. I, p. 24; 1911 ed. vol. I, p. 19.

[45] *Proceedings of the Society of Antiquaries of Scotland*, vol. XXXIV (1899–1900), p. 108.

[46] *The Political History of England*, vol. I, p. 192.

[47] *England before the Norman Conquest* (1910), p. 308.

[48] *The Early Chronicles relating to Scotland* (Glasgow, 1912), p. 90.

[49] *The Mearns of Old* (1914), p. 88.

[50] *The Pictish Nation, Its People and Its Church* (1918), pp. 322 ff.

[51] *Anglo-Saxon England* (1943), p. 88.

[52] To support his identification Chalmers (loc. cit.) refers to a charter (William the Lion to Arbroath Abbey) in which occurs an early form of the name Dunnichen. This important piece of evidence appears again and again in the accounts of later writers.

[53] Frequently repeated is Chalmers's statement (vol. I, p. 210) that the lake 'was long known by the analogous name of Nechtan's mere'. Some later writers emphasize that the site bore this name. It is not impossible that this was so, but it is improbable. *Nechtanesmere* was the English name for the vanished loch and I have found no evidence to suggest that it was ever attached to the site locally.

has been much abused. We should remember that he was the man who first identified *Dún Nechtain* as the modern Dunnichen (54).

Chalmers not only identified the name ; he also identified the fortress itself : ' The remains of this ancient strength may still be seen upon an eminence on the south side of the hill of Dunnichen '. Of the loch by which the battle was fought he added : ' The *Nechtan's-mere* of Simeon was a small lake near the church of Dunnichen on the east, which was drained for its marle or its fuel about forty years ago '. Occasionally it is suggested that Symeon's mere was one of the lochs that lie to the north of Dunnichen Hill (55), i.e. Rescobie Loch, Balgavies Loch, or Loch Fithie. But these lochs to the north lie two miles or so from Dunnichen, and there is no good reason for assuming that a battle bearing the name *Dún Nechtain* and the associated name *Nechtanesmere* was fought outside Dunnichen, on the opposite side of a prominent hill and beside a loch which, so far as is known, has never carried a name associated with the names of the battle as they have come down to us. Chalmers's suggestion is, on general grounds, the more probable, except for the fact that no loch now exists at Dunnichen.

In May 1947, however, there were signs not to be ignored by an observer who was aware that *Nechtanesmere* had once been somewhere in this neighbourhood. There was a shallow, gently sloping basin ; there were patches of sodden marshy ground, and the level of recent floods was marked by faint colour-changes in the growing grasses. Closer inspection revealed traces of embankments, erected at different dates no doubt and reflecting successive attempts to protect surrounding fields from a rise in the level of the waters that once lay in the hollow. Along the southern edge of the hollow runs an old road, now disused but still significantly raised and banked on the side that overlooked what must have been the mere. The line of this road is not accurately represented even on large-scale maps, but its curves are pointless unless it once followed the edge of something. And it is fairly clear that, raised and curving, it once followed the southern edge of a pool or swamp. So much was apparent before references to the mere itself and to draining operations had been found. The old road is now overgrown and almost impassable, but its wall still stands and, when not scrambling through thorns and thistles, one suffers from the persistent illusion that there is something over the wall other than sheep in a fertile meadow, something that no longer exists but something that alone could give a meaning to the structure and direction of the road.

Quite apart from illusions the area is full of interest. There are, for example, traces of a medieval chapel which seems to have been built on an islet, real or artificial, near the northern edge of the mere. Its interest in the present connexion lies chiefly in the fact that it provides a clue to the line of the northern edge in earlier centuries, our immediate object being to form some impression of the ancient limits of the mere. Again illusions, unhelpful this time, intervene. The evidence of recent flood-levels, though suggestive, proved to be no safe guide, for the drains, both effective and ineffective, had modified the natural flow of the waters. The evidence of the embankments was also indecisive for, incomplete and constructed at different dates, they presented only a confused picture. And the hollow itself appeared to change its shape with every change of view-point. To form a reliable impression it was necessary to carry out a field-survey

[54] Chalmers (vol. I, pp. 9, 255) claims that his identification is new. Without an exhaustive study of earlier writers it is unwise to be dogmatic but at least I have found no evidence of any earlier suggestion, no evidence that his claim is exaggerated.

[55] e.g. Thomas Arnold, *Symeonis Monachi Opera Omnia*, loc. cit.

of the area. This involved a considerable amount of work (56), but it provided a series of levels and contour lines which formed a solid basis for map-making when references to the draining of the mere had been collected and arranged.

References to the ' loch ' or ' mire ' are common. Chalmers wrote of ' a small lake ' drained, as he thought, some forty years earlier. James Headrick, minister of Dunnichen, mentioned ' the loch of Dunnichen ' and ' the Mire of Dunnichen ' in 1813 (57) and again in 1833 (58). And in 1831 James Knox (59) mentioned the ' loch, which is now drained '. Other references are not required to prove that a loch existed here. The stages by which it was drained are not so clear. In 1791 the author of the article on Dunnichen in the first *Statistical Account of Scotland* stated that ' it was drained about 30 years ago, and now affords a large supply of peats ' (60). This perhaps led later writers to assume that the loch had disappeared by their time, though in fact it survived for at least half-a-century after 1791. The article on Dunnichen in the First Statistical Account was probably written by George Dempster himself (61), and we cannot reject a statement from such a source. But as it stands it is an overstatement, for the draining of Dunnichen Loch was not a single or a simple operation. John Ainslie surveyed the shire of Angus and his map, published in 1794 (62), shows that the swamp still existed at that time. According to James Headrick, as good a witness as George Dempster, it was only ' drained to a certain extent ' in 1813 and ' it would require more effectual drainage to render the experiment complete ' (63). Twenty years later (1833) Headrick could write : ' The only loch in the parish is what is commonly called the Mire of Dunnichen. This occupies a space of about fifty acres, and has been partially drained for marl, and converted into pasture land. But, to render the improvement effectual, the drain would require to be made five or six feet deeper . . . ' (64). People in the neighbouring village of Letham aver, on second-hand information, that much of the swamp survived for a generation after 1850, but precise and dated descriptions

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[56] To my colleagues, and to students, of University College, Dundee, I am indebted for valuable assistance, advice and supervision : to Professor W. T. Marshall and his staff of the Department of Engineering for the loan of instruments and for instruction, willingly given, on the problems of surveying ; to R. C. Powrie, H. K. R. Allan and S. W. McLeod (graduate students of the Department of Engineering) and to A. R. Baird, D. R. Plummer and D. B. Taylor (of the Department of History) for carrying through the survey with skill and enthusiasm.

A fairly full photographic record was made possible by Dr G. L. Rogers, of the Department of Physics, who gave generously of time, advice and physical effort. It was not considered necessary to reproduce any of our photographs—there are some seventy in all—but they proved to be of great value in the attempt to reconstruct the earlier appearance of the area.

Mr James Millar, owner of East Mains of Dunnichen, kindly facilitated the work at every step. His knowledge, generously placed at our disposal, opened up new possibilities, saved much time and labour, and perhaps saved some of us from injury on the horns of his bull.

[57] James Headrick, *General View of the Agriculture of the County of Angus, or Forfarshire* (Edinburgh, 1813), pp. xi, 40, 63.

[58] *New Statistical Account of Scotland*, vol. xi (1843), p. 146. Headrick's account of Dunnichen was written in December 1833.

[59] *The Topography of the Basin of the Tay*, p. 118.

[60] *The Statistical Account of Scotland*, vol. i (1791), p. 420.

[61] See below, p. 90 n.

[62] *Map of the County of Forfar or Shire of Angus from an Actual Survey made by John Ainslie*, published 1794.

[63] Headrick (1813), op. cit., p. 63.

[64] *New Statistical Account*, loc. cit.

are not available. To-day, with the exception of a single uncultivated strip of some six acres, the loch is productive arable land and pasture. The draining system is still defective, however, and the present farmer advocates improvements very like those suggested by Headrick a hundred years ago. The main outlet is insufficient to carry away the water quickly enough to prevent flooding after heavy rains.

An important stage in the change from swamp to pasture occurred in 1800. Andrew Steele, writing in 1802 (65), tells us that the swamp covered fifty-nine acres, that nearly sixteen acres were trenched and levelled in the summer of 1800, and that in the spring of 1801 this area was ' sown with early oats, except a quarter of an acre planted with yams'. The result was very satisfactory : the yams produced a crop ' more weighty, by double, than on any of Mr Dempster's best fields that season ', and the oats, even without manure, ' were too rank, and therefore lodged '. Steele explains fully how the draining and levelling operations were carried out ; he also gives details of the second year's crop. The value of this crop was not ascertained when he wrote, but he calculates that the profit from two years' cropping was at least equal to the cost (about £69) of the whole improvement. In addition ' sixteen acres of this otherwise useless piece of moss ground ', or £30 per annum in rent value, had been added to the estate. In 1802 grass was sown with the oats and Steele describes the crop as ' most luxuriant ' and ' beyond belief '— so much so that ' it remains doubtful to Mr Dempster, whether, from its appearance of producing so great quantities of good grass, he will let it remain for pasture, or admit it into a due rotation of crops, as part of his farm '.

By describing the operations of 1800 Andrew Steele shows that the draining of Dunnichen Loch was effected gradually and by stages. He also records that draining operations began about forty years before 1800, and thus he supports the 1791 account quoted above. That Steele was not merely following the earlier account, as was Chalmers, is proved by his additional information—he knows that the cost of operations *circa* 1760 was about £50 and he comments upon the various layers of peat, mud, sand and marl revealed by the digging. The 1791 account is therefore properly reconciled with later accounts if we accept it as preserving a reference to the first of several stages in the draining of the area.

George Dempster is the central figure of the story. He was a man whose importance in Scottish and British History is not yet sufficiently recognized : a member of Parliament, an improving and enlightened landlord, a friend of scholars, his wide and enthusiastic interests ranged over the classics, the foundation of new settlements, linguistic studies, road-making in the Highlands, India, feudal services, and the development of fisheries, agriculture, industry, banking, etc. He encouraged and supported Sir John Sinclair's monumental *Statistical Account of Scotland*, and there is very good reason to believe that he was the author of the unsigned 1791 article on Dunnichen, which appeared in Sinclair's first volume and which has been mentioned above (66). He was born in 1732, and he held the Dunnichen estate from 1754 until his death in 1818. To this half-century belong the earliest and most important stages of the draining of

[65] *Account of the Peat Mosses and Marl on the Estate of Dunnichen, in the County of Forfar*, Appendix E, pp. 73–8, of James Headrick's *General View of the Agriculture of the County of Angus*, cit. sup.).

[66] p. 89. *The Letters of George Dempster to Sir Adam Fergusson* (ed. James Fergusson, 1934) provides the fullest account of George Dempster and his interests. This scholarly work does not claim to be a biography, but its author has transformed a mass of unpublished material into a vivid picture of Dempster, a picture which emphasizes his historical importance no less than his personal charm. See p. 202 n. for the suggestion that Dempster wrote the 1791 account of Dunnichen.

Dunnichen Loch. The use of marl for agricultural purposes in Angus became common during the 18th century, and the extraction of marl from the beds of lochs was apparently first attempted on a large scale about 1730 (67). Several lochs in the district of Forfar were drained, some with this object in view (68). George Dempster was naturally interested, and his great achievement in this direction was the draining of Restenneth Loch in 1788–89 (69). By 1792 he could boast: 'I am now the only marl and peat merchant to any amount in the county' (70), and as he seems to have gained about £1000 per annum from the Restenneth venture (71) his boast and his satisfaction were not unjustified. The draining of Restenneth Loch was his pride, but Dunnichen Loch had given him experience thirty years earlier.

Much incidental information is available for the study of George Dempster's local activities and for the study of marling and other aspects of agricultural development in Angus. Our concern is with Dunnichen Loch, and the main stages of its transformation are tolerably clear. About 1760 the first serious attempt at draining was made by George Dempster. Though some marl was obtained (72), the chief object was the 'procuring more easily peats for fuel' (73). During the next forty years fuel pits were dug in many places, and the drained area 'acquired a sward of course grass'. Then came the operations of 1800; peat and marl were both obtained no doubt, but land reclamation seems to have been most prominent in George Dempster's mind. The later stages, gradual shrinkage and improved drainage during the 19th and 20th centuries, have at last produced the 'rich meadow' which Dempster prophesied in 1791 (74).

The various lines of evidence may now be drawn together. The above accounts and references give a fairly clear impression of what the loch or swamp was like in area and appearance at different dates during the past two hundred years. From field-work we get accurate levels and contours as well as a general familiarity with the physical geography of the district. It therefore becomes possible to draw *Nechtanesmere* on a modern map with some confidence in the result (see map). Our estimate of shape and size is controlled by the known historical facts and by the data derived from the field-survey. It is closely corroborated by Ainslie's Map of 1794. Further corroboration, after the typescript was complete and after the map was drawn, came from an air-photograph on which the area of the vanished loch was clearly indicated by a marked discoloration of the ground invisible except from the air.

Nechtanesmere occupied a shallow valley. Levels show that it can never have been a deep loch, though in parts there would often be ten or twelve feet of water. There was an outlet at the southeastern end, and the overflow passed through what is now the village of Letham (cf. Mossburn Street) and joined a stream called the Vinny, which itself joined the Lunan Water and entered the sea in Lunan Bay between Arbroath and

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[67] James Roger, *General View of the Agriculture of the County of Angus or Forfar* (Edinburgh, 1794), pp. 24–5.

[68] James Roger (1794), loc. cit.; Andrew Steele (1802), loc. cit.; James Headrick (1813), op. cit. pp. 85 ff., 388 ff.

[69] Cf. *Letters of George Dempster*, cit. sup., pp. 174, 179, 193, 196, 202; Andrew Steele (1802), loc. cit.; James Roger (1794), loc. cit.

[70] *Letters of George Dempster*, p. 218.

[71] ibid; Andrew Steele, op. cit., p. 73.

[72] See above, p. 89.

[73] Steele, op. cit. p. 75; see also above, p. 89.

[74] *The Statistical Account of Scotland*, vol. I (1791), p. 420: 'In all probability it will furnish the neighbourhood with fuel for about 30 years longer, and may then be made a rich meadow'.

Montrose. The edges of peat mosses tend to rise as they dry out, and the loch was probably greater in extent than the modern survey suggests. Its origin should be sought principally in a very vigorous spring and in surface waters which fall from the surrounding higher ground. Several valleys in the Sidlaws once held lochs similarly formed (**75**). The spring still exists (see map), and even in the driest seasons it pours out a great volume of water. The contribution of surface waters would be very considerable, especially after heavy rains, for the basin drains a wide area including Dunnichen Hill. The underlying layer of marl and the porous nature of the upper soils also tended to keep the hollow always waterlogged. These conditions, together with an outlet that was insufficient rather than non-existent, fully explain why a sheet of water should lie stagnant in this valley. The extent of water as distinct from marsh no doubt varied very much with the seasons, but the average depth was never great. It was a swamp rather than a loch, a mire rather than a mere.

It is not known exactly where the battle of 685 was fought. During the past two hundred years a surprising amount of archaeological material has been recovered from fields near the swamp, especially from fields immediately to the north of the swamp, and several writers have attempted to associate this material with the battle. No such association seems permissible, although on general grounds it is not unlikely that the battle took place in this area between the swamp and the hill. The 1791 survey of Dunnichen in the First Statistical Account of Scotland (**76**) tells of ' antient tumuli or barrows ', containing ' human bones ' and sometimes ' pots of a coarse earthen-ware ' in ' rough stone coffins '. Its author discreetly refrains from speculating on the date of these finds, and, knowing nothing of the supposed site of *Nechtanesmere*, he naturally makes no allusion to the battle. Chalmers (**77**) quotes this account and implies that the finds are related to the battle. Knox (**78**) follows Chalmers, adding only emphasis : ' The field of this battle is marked by several *tumuli* and cairns, some of which, on being opened, were found to contain human bones in rough stone coffins '.

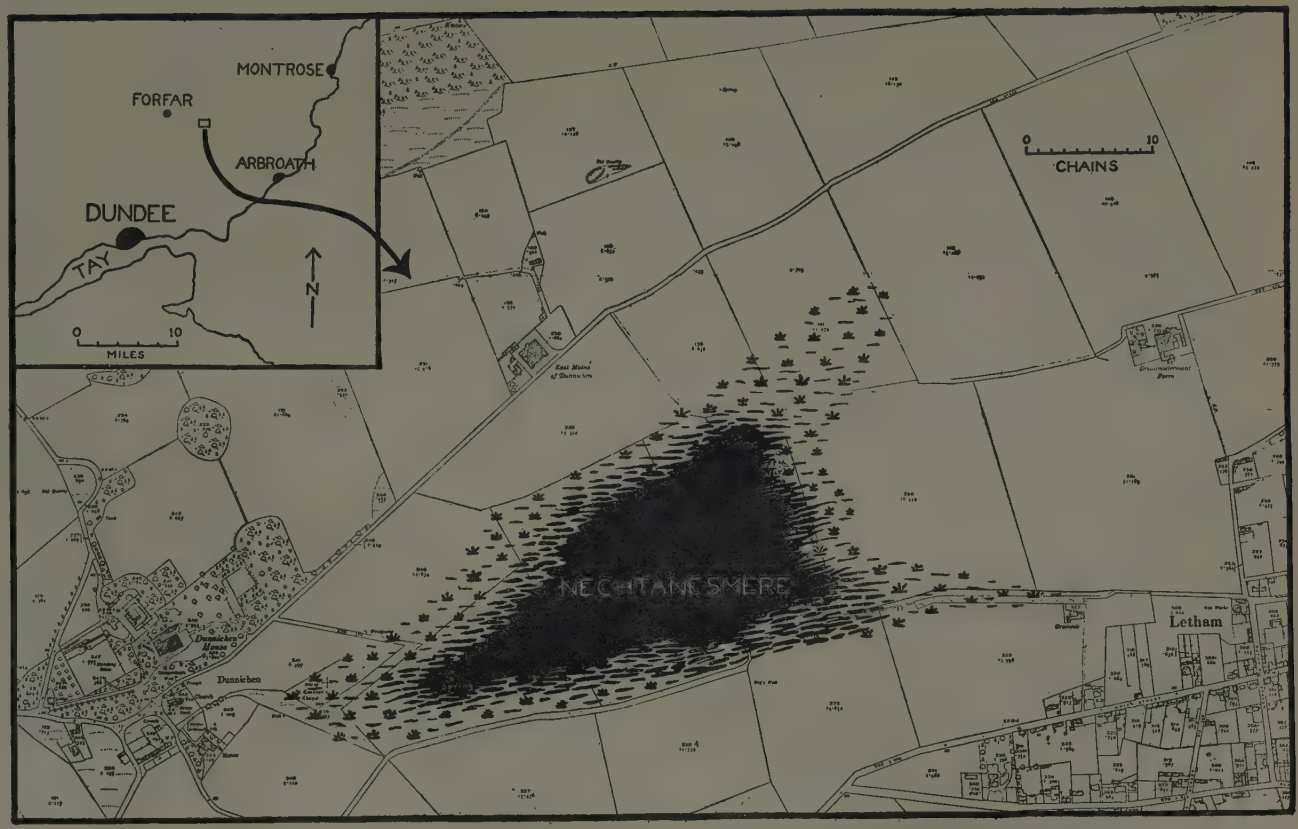
A fuller survey is given by Headrick (1833) in the Second Statistical Account of Scotland (**79**); he tells us that on the East Mains of Dunnichen graves have been found, filled with human bones and decorated urns of red clay. Unfortunately, ' the bones and the urns mouldered into dust ' upon exposure to the air. Similar ' graves ' were found in ' a round gravel knoll near the Den of Letham ' (about a mile to the east); these too contained bones, ' crammed together without much regard to arrangement ', urns, and strings of glossy black beads. Again all have ' mouldered into dust '. In some of the graves were ' rusty daggers ', but these ' fell in pieces by handling '. Headrick concludes from this material that ' some battle has been fought here ', but he mentions only a confused tradition which involves King Arthur—he ignores *Nechtanesmere* and so silently rejects Chalmers's identification (**80**). Later writers quote Headrick's account directly

[75] ' Many of these have been so far drained as to draw the water from the surface; though it regurgitates upon some of them during wet weather. Were the main outlet made a few feet deeper and broader, and were concealed drains thrown round the margin of such valleys, where the springs break out, these unseemly swamps would be converted into very fertile land ' (James Headrick, 1813, op. cit. pp. 394–5). ' There are various tracts of hollow marshy ground, which were formerly lakes, but have been either wholly, or partially drained ' (ibid, p. 86).

[76] cit. sup., vol. I, p. 429. [77] op. cit. vol. I, p. 255 note. [78] op. cit. p. 118.

[79] *New Statistical Account of Scotland*, vol. XI, pp. 146–7.

[80] That Headrick was aware of Chalmers's work is proved by his earlier (1813) expressed disagreement with Chalmers's derivation of the name Dunnichen (*General View of the Agriculture of the County of Angus*, p. 163).



Reproduced from the Ordnance Survey 25 in. Map, Forfarshire, sheets 39/6, 39/10, with the sanction of the Controller of H.M. Stationery Office

or indirectly and associate these finds more or less firmly with the battle of 685. To give a single example, Marshall (81), apparently unaware that his path is well-trodden, describes what to him were ' obviously the hoarded spoils of war ' and rhetorically asks if it is not ' quite likely that they were the memorials of the Battle of Dunnichen '. We must answer him firmly in the negative. We cannot examine what has mouldered into dust, but Headrick's ' graves ' appear to have been typical cist-burials of a much earlier age. Though the possibility of secondary interment should not, perhaps, be entirely ignored, it is safe enough to deny any obvious connexion between this lost archaeological material and the Battle of Dunnichen. The battle seems to have left no archaeological trace of itself, which is not in the least surprising.

Battle-sites, unlike fortresses and dwellings, are notoriously elusive ; they seldom leave clear traces of themselves for the archaeologist, and it is doubtful if the use of modified mine-detectors in the future will do much to reveal the many unknown sites of battles. Neither victors nor vanquished would normally leave valuable weapons and other loot lying on the field of battle, and what remained would be thoroughly gleaned by the local population. The battle of 685 was probably a running fight in which pursuit merged into flight, and it would probably be a waste of time to seek a battle-site as the term is generally understood. The most one could expect to find would be a mass of human remains buried without much ceremony. It might be argued that the ancient cists at Letham were used as a burial place for some of the slain—note Headrick's statement that the bones were ' crammed together without much regard to arrangement '—but it would be unwise to build a complicated theory of secondary interment upon a single statement concerning evidence no longer available for examination. In any case the swamp probably claimed many bodies—dead as well as alive.

The *dun*, or fortress, of Nechtan, commemorated by Irish annalists and by the place-name Dunnichen, is less elusive than the site of the battle, but traces of it to-day are neither conspicuous nor unambiguous. In 1791 it was recorded that ' on the south side of the hill is an eminence, now called Cashells or Castle-hill, with visible remains of the foundation of some ancient building ' (82). Chalmers (83) quotes this statement but gives the name as *Cashill* or *Castle-hill*. In 1813 Headrick (84) tells us that the fortress ' has been demolished for stones to build fences ', and in 1833 he adds that ' its area has been nearly obliterated by a quarry ' (85) which he calls ' the castle quarry of Dunnichen ' (86). Jervise gives the name variously as *Chasel*, *Cashel*, and *Castle Park* (87). A hundred and fifty years ago the ' fortress ' must have been conspicuous, but demolition and quarrying have wrought such devastation that people now living below the site are unaware that any structure ever existed there. Remains are still visible, however : a few stones and the broken line of what seems to have been a stone wall or earthwork. They stand on a small plateau which has its own water supply and which projects from the south side of the hill near but below the summit. Excavation, if not already too long postponed, may later throw clear light upon their origin and function, but it may be noted now that the main features of this site are repeated in other sites in this part of Scotland. Similar sites in Angus and Fife have been examined and, in one or two cases, excavated, and they appear to be Pictish settlement-sites of the Dark

[81] *Historic Scenes in Forfarshire*, p. 119.

[82] *Statistical Account of Scotland*, vol. I (1791), p. 419.

[83] op. cit. p. 255. [84] op. cit. (1813), p. 163.

[85] *New Statistical Account*, vol. XI (1843), p. 146. [86] ibid, p. 144.

[87] *Proceedings of the Society of Antiquaries of Scotland*, vol. II (1854–7), p. 189 ; *Memorials of Angus and Mearns* (1861), p. 21.

Ages. Until more is known of these sites in general and of this site in particular it would be unwise to strain after a final conclusion, but it certainly looks as if the mutilated remains on Dunnichen Hill represent a Pictish settlement.

Especially interesting are the names quoted above. *Castle-hill, Castle Park* and *Castle Quarry*, though usually stressed by writers, are probably no more than Anglicized forms of *Cashel, Cashill* and *Cashells* which appear to represent Gael. *caiseal* (Ir. *casel, caise(a)l*, ' a stone wall, a circular stone fortress '). These names are significant, and they undoubtedly refer to the structure that once stood on Dunnichen Hill. It seems probable that this site, possessing the general characteristics of Pictish settlement-sites so far as they can be recognized at present, was a village-settlement surrounded by a protecting wall. Like similar sites, it was not planned as a strong defensive position—it is not even on the top of the hill—but rather as a walled and semi-protected settlement. Excavation might reveal nothing on a site so decayed and damaged ; one might guess that it would probably not render untenable the view that this site was occupied before and during the 7th century, that it is in fact the *Dún Nechtain* of the Irish annalists. At least there is nothing improbable in the conclusion to which the very meagre evidence seems to point.

Nothing definite is known about the route followed by the Northumbrian army. Chalmers and Skene paint vivid pictures of Ecgfrith's advance which, they say, was marked by the burning of *Tula Aman*, placed by Skene ' at the mouth of the river Almond where it falls into the Tay ', and by the dispatch of ' a detachment from his army into Dalriada, where he burnt Duinollaigh, now Dunolly '. *Tula Aman*, person or place, may refer to the River Almond in Perthshire and even perhaps to the Roman station known to have existed in this neighbourhood ; *Dunollaigh* is probably correctly identified as Dunollie near Oban. But the reference to *Tula Aman* and *Dun Ollaigh* is preserved only in the *Annals of Ulster* and there in a curious form. Following upon the mention of the Battle of Dunnichen and the death of Ecgfrith comes the phrase : 7 *combusit tula aman duin Ollaig*[*h*] (88). It will not easily bear the translation imposed upon it by Skene, and its meaning is obscure (89). Its appearance in the *Annals of Ulster* alone of the major Irish chronicles raises the suspicion that it did not form part of their common original. It may be an accidental interpolation—Dunollie was burnt or destroyed on more than one occasion (90), and a scribe might easily record the same event more than once. In any case the phrase, even if it is not an accidental interpolation, may not apply to Ecgfrith's expedition. If it is no mistake and if it does apply to Ecgfrith's expedition, which is perhaps unlikely, its meaning is still obscure. We certainly cannot build any theory on it. In the absence of any evidence we should assume that Ecgfrith crossed the Forth near Abercorn or Stirling and the Tay near Perth. But this is only an assumption. There is no evidence of a naval operation, and that should be ruled out as improbable.

From Perth northwards two possible routes lay open. Ecgfrith could advance straight up Strathmore, between the Grampians and the Sidlaws. Or he could proceed along the Carse of Gowrie, keeping between the Sidlaws and the Tay. The former route would involve a turn south somewhere about Forfar, for Dunnichen lies about three miles south-east from Forfar ; the latter would involve a break through the Sidlaws

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[88] This phrase follows *interfectus est* (see above, p. 85). In the *Annals of Tigernach* its place is taken by *la Bruidhi mac Bili regis Fortrenn* (' by Brude son of Bile the king of Fortriu ').

[89] See A. O. Anderson, *Early Sources of Scottish History*, vol. I, p. 193.

[90] See *Annals of Ulster, sub annis* 685, 697, 700.

to the north not far from Dundee which is some fourteen miles from Dunnichen. There is no evidence that the Northumbrians met organized Pictish resistance before the day of the fateful battle, and there is no obvious clue as to how they approached Dunnichen. Wandering over this part of the country with Bede's brief account in mind one receives impressions, but unfortunately the impressions favour first one route and then the other. As there is no evidence it is perhaps worth while to record the impression of the moment.

I favour the Strathmore route. In the first place there was a good road through Strathmore. Secondly Bede's reference to ' the narrows of inaccessible mountains ' fits Strathmore more closely than it fits the road along the Carse. Thirdly, if Ecgfrith was drawn off his route by a Pictish retreat, genuine or feigned, it is difficult to imagine wild pursuit sustained over fourteen miles of enemy country ; the flight and the pursuit are likely to have been short and sudden. Flight from the north to the south would be short and would perhaps bring the armies through the cleft in Dunnichen Hill which admirably suits Bede's phrase ; flight from the south towards Dunnichen would be across broken but by no means difficult or mountainous country. Finally, from the north the fortified settlement on Dunnichen Hill would be quite hidden from the pursuers until they were upon it ; from the south it would be visible for several miles, and one cannot easily believe that an army with that potential menace in view would not have checked or controlled its pursuit. One can imagine the Northumbrians bursting through the cleft in the hill and realizing too late that Nechtan's fortress, and possible reinforcements for the enemy, lay three or four hundred yards to their left. Pursuit would easily turn to flight—flight downhill towards the mere at the bottom. This is speculation, of course, but it is clearly labelled.

To assess the significance of the Battle of Nechtanesmere would require a fuller survey of northern history than is possible within the limits of this article. Bede has summarized the immediate results :

> *Ex quo tempore spes coepit et uirtus regni Anglorum fluere ac retro sublapsa referri. Nam et Picti terram possessionis suae quam tenuerunt Angli, et Scotti qui erant in Brittania, Brettonum quoque pars nonnulla libertatem receperunt ; quam et hactenus habent per annos circiter* XLVI *. . . Successit autem Ecgfrido in regnum Aldfrid, uir in scripturis doctissimus, qui frater eius et filius Osuiu regis esse dicebatur ; destructumque regni statum, quamuis intra fines angustiores, nobiliter recuperauit* (91).

Many Angles were slain or enslaved. Amongst those who fled south was Trumwine, Bishop of the Picts under Northumbrian lordship. His Pictish see came to an end. Recent Northumbrian advances towards the control of the Picts, the Britons, and the Irish of Argyll, were obliterated, but Aldfrith, Ecgfrith's successor, ably restored—within narrower limits—the Northumbrian kingdom which, though shaken, was by no means shattered.

The Northumbrians were compelled to withdraw but, since we do not know the precise extent of their advance during the half-century before 685, we do not know the precise extent of their withdrawal. A vague overlordship of all Britain was attributed to Edwin, Oswald and Oswiu, but it is difficult to believe that any Northumbrian king of the 7th century exerted effective control in the remote north. Oswiu and Ecgfrith had undoubtedly pushed forward to annex lands and to bring some of the Pictish, British and Irish peoples under a control which, though it probably did not extend to the far north, was certainly more than a vague and disputed claim to lordship. So much is clear from Bede's summary. The withdrawal of the Northumbrians still left them with

[91] *Historia Ecclesiastica*, cit. sup., Lib. IV, Cap. 24.

the Forth as a frontier. It may have been an insecure frontier for some years after 685 but it was maintained throughout the 8th century and into the 9th.

Before the disaster of 685 the Northumbrians were clearly the dominant and aggressive power in the north. Early in his reign Ecgfrith had successfully suppressed a Pictish rising, and in 684, one year before the Battle of Nechtanesmere, he had sent an army to devastate part of Ireland—possibly because the Irish of Argyll had drawn or might draw support from that quarter. The defeat of 685 was a serious reverse for Northumbria : it destroyed the ascendancy built up by Oswiu and Ecgfrith. But it was not in itself a fatal reverse : Aldfrith was able to consolidate the kingdom within its ancient limits, by 711 the Northumbrians were again able to defeat the Picts in battle, and their most glorious achievements in art and literature were still in the future.

It is easy to exaggerate the historical importance of the battle. It is traditionally regarded as the turning-point in Northumbrian fortunes, as marking the transference of political supremacy in the north from the Northumbrian to the Pictish kings, and as scoring across History the decision that what is now Scotland should be essentially a Pictish not an English kingdom. To believe this is to ignore later history and to confuse cause with effect. To say that the north would have become an Anglian province if Ecgfrith had won the Battle of Nechtanesmere is less accurate than to say that the north might have become part of the Northumbrian kingdom if Ecgfrith and the Northumbrians had had sufficient strength to win the Battle of Nechtanesmere. But they had not sufficient strength to meet and defeat the Pictish hosts in hostile territory and far from their own bases. The fact is that Northumbria was not strong enough to maintain the position Ecgfrith and Oswiu had won in the north. The battle should be regarded as an illustration of this fact, not as its cause. It provides a measure against which Northumbrian strength may be judged, and it sets a clear chronological limit to Northumbrian expansion, but it did not cause the decline of Northumbria. As noted above, the decline of Northumbria did not preclude later Pictish defeats and did not deprive the Angles of their traditional frontier. The Picts certainly rose under Brude mac Beli to a political eminence without precedent in their recorded history, but the decline of Northumbria should not be attributed to the rise of any northern people.

The decline of Northumbria, later accentuated by the failing effectiveness of a central government lacking dynastic stability, was due chiefly to the rise of the strong midland kingdom of Mercia. The career of Penda foreshadowed the danger inherent in the consolidation of the Mercian kingdom. Northumbria could not cope with it. Oswiu slew Penda and for a time ruled Mercia, but Penda's son Wulfhere restored Mercian independence and robbed Oswiu of the overlordship of the southern English peoples. It seems true enough to say that Oswiu turned from the south to satisfy his ambitions in the north, but he could not avoid Mercian rivalry and its effects merely by turning away from the south. Nor could his son Ecgfrith ; he too was compelled to meet the Mercians in battle. The future lay with Mercia ; the Northumbrian state had already before 685 started along the downward road which ultimately led to the collapse of 867.

It was a long road but the Picts do not stand at the beginning of it. The Northumbrians had built up in the north a position which, harassed by Mercian rivalry, they had not the strength to maintain. Of this the Battle of Nechtanesmere is an illustration rather than a cause. It reflects rather than changes the true political balance in the north. We see for the first time a definite limit set to Northumbrian expansion, a limit already overstepped by Oswiu and Ecgfrith.

Reflections on Collingwood's *Idea of History*

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PROFOUND books are always worthwhile. If we agree with them, their ideas become a direct contribution to our knowledge. If we disagree, they impel us to examine the basis of disagreement and to develop our own counter-ideas, and thus they make an indirect contribution to our knowledge. R. G. Collingwood's *The Idea of History* (Oxford, Clarendon Press, 1946) is the most penetrating work on the foundations of historical method known to me, but I find it unacceptable. My major difficulties result from his ontological idealism and his dichotomy between mind and nature.

Collingwood was ontologically an idealist. To an idealist a thing exists insofar as one has an idea of it. Therefore the past exists insofar as one has an idea of it. Therefore the past exists as one's present conception of it.

> . . . the past is not a dead past but lives on in the present, the historian's knowledge . . . is knowledge of the past in the present, the self-knowledge of the historian's own mind as the present revival and re-living of past experiences (p. 175).

He takes an idealistic approach to social phenomena :

> . . . for a man who proposes to act, the thing that is important to think about is the situation in which he stands . . . the situation consists altogether of thoughts, his own and other people's (p. 316).

History then becomes the history of ideas :

> All history is the history of thought . . . The history of thought, and therefore all history, is the re-enactment of past thought in the historian's own mind (p. 215).

In opposition to Collingwood, for an ontological materialist a thing exists in itself irrespective of whether anyone has an idea of it. Therefore there is a past-in-itself, as it were, which is the past ' in its entirety ' (p. 327). Our present conception of it is our past-as-known. The historian's past-as-known consists of his inferences based upon the facts available to him.

A consistent ontological materialist also takes a materialistic approach to social phenomena. Thus, to paraphrase Collingwood, for a man who proposes to act, the thing that is important to think about is the situation in which he stands, which consists altogether of the environment and social acts, his own and other people's. Moral ideas are conceived, fundamentally, as the expression of social institutions. As for human history, it is the narration of a succession of relevant social and cultural events.

From the materialistic point of view, Collingwood's conception of history raises many problems. Some seem implicit in the following :

> . . . not only do we understand Greek mathematics easily enough, it is actually the foundation of our own. It is not the dead past of a mathematical thought once entertained by persons whose names and dates we can give, it is the living past of our own mathematical inquiries, a past which, so far as we take any interest in mathematics, we still enjoy as an actual possession (pp. 225–6).

I would deny that it is easy to understand Greek mathematics the way the Greeks understood it. The Greek conception of the axioms of Euclidean geometry, for example, is different from our conception of the set of postulates of a deductive mathematical system. It is not easy for us to see the philosophic and religious significance of the theorem of Pythagoras attributed to it by the Greeks ; and I doubt if many students of Greek mathematics do. By diffusion some parts of Greek mathematics were adopted by Western European culture, but in the process they have been modified in form, use, and meaning. Therefore to say that the history of Greek mathematics is our actual possession of the subject is not to discriminate between the Greek's conception of their mathematics and our conception of that part of our own mathematics which we have adopted from the Greeks. Collingwood does not seem to distinguish between the past-in-itself, tradition, and the historian's conception of the past. By tradition we usually mean the customary, or layman's past-as-known ; it includes mythology and legend. The historian's past-as-known is presumably composed of inferences about the past based upon facts from that past. It is a truism that every generation writes its own history. Does that imply that the past changes with every generation ? A materialist would argue that the past-in-itself cannot change, but that the past-as-known can and does. Therefore it is not the past-in-itself that lives on in the present, but the past-as-known.

Collingwood then goes on to make a dichotomy between mind and nature. Insofar as the human mind is unique, it must be studied in a way different from that used in studying nature. This is his essential distinction between history and science.

> . . . the processes of nature are not historical processes and our knowledge of nature . . . is not historical nature (p. 302).
>
> Of everything other than thought, there can be no history (p. 304).

From the point of view of a materialist man is a part of nature and therefore subject to what are commonly called the ' laws of nature '. Everything in nature has certain qualities of its own, and those of man include certain unique kinds of ' mental ' activities, but the latter are as much a part of nature and subject to its ' laws ' as the honey-making activities of the bee. As students of man we may be interested in the qualities peculiar to man, but I fail to see how this differs from the apiologist interested in the qualities peculiar to honey bees. History then becomes any narration of a succession of relevant events. To use the example given by Collingwood (p. 115), the historical geologist gives us a series of events known as geologic events, while the human historian gives us a series of events known as human activities. It follows that for Collingwood history and science are separate ways of knowing.

> . . . the historian discovers . . . the autonomous character of historical thought, its power to solve its own problems for itself by its own methods. He discovers how unnecessary it is, and how impossible it is, for him, as historian, to hand these problems over for solution to natural science . . . Historical thought, thought about rational activity, is free from the domination of natural science, and rational activity is free from the domination of nature (p. 318).

In opposition to this point of view, it seems possible to argue that history and science are different but interdependent facets of the same subject.

The historian is interested in the succession of a particular series of events, while the scientist is interested in laws, i.e., relations between classes of events (see pp. 205–6). This difference is shown in the types of explanation offered by historians and scientists. An historical explanation is the narration of the sequence of relevant events leading up to the event to be explained. Scientific explanation is making the event a member of a

class of events (see p. 214). Here I think Collingwood overlooks the implications of one of his own analyses.

> I may now be experiencing an immediate feeling of discomfort, and I may ask myself why I have this feeling. I may answer that question by reflecting that this morning I received a letter criticizing my conduct in what seemed to me a valid and unanswerable manner. Here . . . I am recognizing in its detail a certain individual event or series of events (p. 174).

In my estimation, what he has done here is give an historical explanation. A scientific (psychological) explanation might be that this is a case of the generalization—frustrated aggression produces discomfort. I submit that both are valid explanations.

History and science are also interdependent. This historian provides the data for the scientist, while the scientist provides the frame of reference for the historian, and the findings of each may modify the other. If a social scientist wants to discover generalizations about revolutions he goes to history for the data upon which to base his generalizations. And when a historian is studying the history of the French Revolution he depends upon implicit or explicit generalizations on four counts. First, he must decide what data are relevant out of the welter of facts available to him, and it is his hypotheses about the nature of revolutions which influence his choice of certain facts as significant and others as insignificant. Second, the historian critically examines and interprets his data.

> Thus, a commander's dispatches may claim a victory ; the historian, reading them in a critical spirit, will ask : ' If it is a victory, why was it not followed up in this or that way ? ' and we may thus convict the writer of concealing the truth (p. 237).

Is not the historian here making a judgment in terms of some generalizations about the relation between classes of human events, which these dispatches contradict ? And if he goes further and says that this was due to the commander's desire to save face, is his conclusion not based on a generalization about how people save face in certain situations ? If the answers are affirmative, then criticism and interpretation of historical data depend upon implicit or explicit generalizations. Third, Collingwood speaks of a methodological process he calls ' historical construction '.

> The historian's authorities tell him of this or that phase in a process whose intermediate phases they leave undescribed ; he then interpolates these phases for himself. His picture of his subject drawn from his authorities, consists also . . . of statements reached inferentially from those according to his own criteria (p. 237).
>
> [Thus] we find ourselves obliged to imagine Caesar as having travelled from Rome to Gaul when we are told that he was in these different places at successive times (p. 241).

It seems a painful elaboration of the obvious to say that this ' construction ' is inferred on the basis of knowledge of a generalization of the relation between certain classes of events, specifically, that when a man is at one time in one place, and in another at another, he has travelled from one to the other. Fourth, when the historian gives the sequence of relevant actions which constitute his explanation of an event, the question arises, How does he know that his explanation is the true one ? There are many historical explanations of the French Revolution. To the extent that the historian remains in the realm of a unique series of particular events such as the French Revolution, on what basis can he argue in favour on one explanation rather than another ? It seems to me that this can only be done in terms of generalizations about revolutions, which in turn have been verified by the comparative or experimental method. That is, the historian

would have to show that his own explanation is based upon valid generalizations about revolutions, while the alternative explanations are based upon invalid ones. It is possible to go further and argue that no historical explanation is any more valid than the scientific generalizations upon which it is based. Since the time of Hutton historical geology has been inferred in terms of generalizations regarding modern geological processes. Similarly, inferences in human social and cultural history are based upon our knowledge of the laws of social and cultural processes. Is not this whole analysis implicit in such passages by Collingwood as follows :

> The historian not only re-enacts past thought, he re-enacts it in the context of his own knowledge (p. 215).
>
> . . . the interpreting of evidence is a task to which a man must bring everything he knows : historical knowledge, knowledge of nature and man, mathematical knowledge, philosophical knowledge (p. 248).

Much of the difficulty lies in the ambiguity of Collingwood's ' fact '. In his use of the term,

> [Scientific] facts [are] . . . facts directly observed by the scientist . . .
>
> In history, the word ' fact ' bears a very different meaning . . . It is arrived at inferentially by a process of interpreting data (p. 133).

In other words, for Collingwood a scientific fact is ' something immediately given in perception ' (p. 132), while an historical fact is an inference.

In opposition to Collingwood, it could be argued that the historian and the scientist use the same facts. Neither is interested in all of the crude facts, i.e., all phenomena in all their complexity, as experienced. They are only interested in those facts, and further, only in such aspects of those facts, which are relevant to their problem, and it is these relevant aspects of relevant facts which constitute their data. In both cases relevance is determined by the nature of the problem and the frame of reference. The way they perceive these data is also determined by their frame of reference, and such perception itself is an inference. This can be seen by such an everyday occurrence as walking down the street, thinking we see a friend, hailing him, and on coming closer finding that he is a stranger. If this is so, it follows, to use Collingwood's example (p. 244), that an instance of an historical datum would be ' a pattern of black marks on white paper '. The historian perceives (i.e., interprets) it as ' Greek letters ; that the words which they form have certain meanings in the Attic dialect '. He infers ' that the passage is authentic Thucydides, not an interpolation or corruption ; and that on this occasion Thucydides knew what he was talking about and was trying to tell the truth '. The human historian observes an artifact such as a manuscript and makes certain inferences about the Peloponnesian war ; the physicist observes rings on a metal foil and makes certain inferences about the atomic conditions which produced them. If anyone doubts either inference he can go to the facts and form his own conclusions. The facts for the human historian are material objects from the past (artifacts) and living people's observations (their memories of past observations and their present observations in the case of contemporary history). The facts for the scientist are likewise objects (remains of past events) and living scientists' observations (their memories of past observations and their present observations). And as has been said before, neither the historian nor the scientist is interested in crude facts as such. In both cases they deal with data—relevant aspects of relevant facts—from which they make inferences (see p. 234). These inferences may be said to be mental constructs. No living historian witnessed the Peloponnesian war, but neither has any physicist seen an atom.

As I see it, the activity of an organism can be analyzed into motives, acts, and goals. The motive is the tension which impels the organism to act. The acts are the means by which the organism tries to reach a goal; some of these are covert, taking place within the organism, and others are overt, consisting of the external adjustments of the organism to its environment. The goal is that which satisfies the motive. For instance, while working I become hungry (motive), decide it is time for lunch (covert acts), go to the dining room (overt acts), and eat (overt acts) some food (goal). Now, usually all that the observer sees are some of the overt acts and goals, from which he infers the motivation and covert acts, thus reconstructing the entire activity in order to understand what he observes (see pp. 213–16). Now let us shift to an inorganic illustration. The atomic scientist sees rings on a metal foil, from which he infers a previous set of atomic events, thus reconstructing the entire series of events in order to understand it. What is the difference between the two cases that makes the understanding of human activities so different from the understanding of atomic phenomena? If the answer is that we can exercise what Collingwood calls 'sympathetic insight' in the former case and not in the latter, my answer is that this is simply the advantage of human beings studying human phenomena, and the scientist of human activity (e.g., the psychologist) uses it as much as does the historian of human activity.

Finally we come to the problem of verification. How do we tell whether an historical or scientific inference is valid? I would argue that in both cases it is by means of the two criteria of consistency and prediction. A proposition is likely to be true if it is not contradicted by the available valid data. The historian rejects Suetonius' statement about Nero and accepts that of Tacitus (pp. 244–5); similarly the scientist rejects 'faulty' observations and accepts those which seem accurate. A proposition is more likely to be true if consequences can be drawn from it which are found to occur in the future. The Einsteinian theory inferred the deflection of stars in the sun's gravitational field, and these were observed in subsequent solar eclipses. An historian infers that a certain event happened in Roman history and later an inscription is excavated which deals with that event. Of course, in both history and science observations relevant to predictions are not accepted any more uncritically than those relevant to consistency. Some observations of the deflection of stars have been rejected as faulty, just as some inscriptions have been rejected as spurious (see p. 243).

I have dealt with only a few of Collingwood's basic ideas; the innumerable insights which he presents in this book cannot be considered within the limits of a paper. What I have tried to show is that other metaphysical assumptions than his can be made, and that such assumptions have implications for social theory and research.

Notes and News

ARCHAEOLOGY AND TOPOGRAPHICAL INTERPRETATION OF AIR-PHOTOGRAPHS

The three years since the close of hostilities have brought forth a spate of articles on archaeological air-photography based, for the most part, on material made available from Service sources. All these hitherto have dealt with archaeological remains which actually show as such from the air. However, for a variety of reasons, many sites remain unseen from the air but the topographical features which dictate their location may be discerned on an air-photograph and yet not appear on the map. As far as the writer is aware no attempt has been made to apply air-photography to the location of these ' unseen ' sites and three examples, one in Australia and two in Malaya, are discussed below.

Early visitors to Port Phillip Bay, a large land-locked gulf in southeast Australia, at the north of which the city of Melbourne now stands, commented on the number of aboriginal summer camps. In March 1947, it was suggested to the writer that air-photographs might be employed to locate some of those camp sites. Elsewhere in South Australia and Western Victoria extensive middens had been located from the air with some success.*

The area centring on Point Cook (Lat. 37° 55′ S; Long. 144° 48′ E.) was selected for examination partially because the area was undeveloped and partially because numerous finds had been made in that region although the exact extent of the camp sites was unknown.

A preliminary study of air-photographs proved disappointing—no archaeological sites or middens as such being observed. On the other hand the presence of a series of beaches, both above and below the present high water-mark, was noted for the first time.

Now it is obvious that the requirements of a nomadic people forming a summer camp in such an area would be threefold—namely :—

1. Reefs or a wide extent of beach uncovered at low tide to allow easy collection of marine food.
2. Fresh water readily available.
3. Easy access to the beach.

The presence or absence of reefs or extensive beaches is the decisive factor, and since these camps were in existence in recent historic times it is unlikely that there would be any great changes in the beach features, especially in an enclosed bay. Examination of the standard one inch to the mile Australian Army Survey Corps map (South J 55, G IV SE and SW, Victoria, Melbourne) showed certain features which are tabulated below. (Present limitations of paper do not permit reproduction of this map).

1. An apparent absence of reefs, since the 1 fathom line and the low watermark run at a fairly constant distance from the back of the beach except off the village of Altona.

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*Information from Norman B. Tindale, South Australian Museum, Adelaide.

2. Various streams and marsh areas providing plentiful fresh water and probably waterfowl.
3. Marshes restricting beach approaches to three areas—Altona, Point Cook and Point Cook Airfield.
4. No apparent difficulty in reaching the beaches in these three areas.

Now each of these areas embraces a considerable length of beach and the object in considering the air aspect was to limit field work to the most desirable areas for camp sites. Air-photographs at once showed reefs at all three sites. Point Cook itself is illustrated in the Plate and may be regarded as fairly typical of the other two sites. The reef here is partly overlaid with sand and appears as a roughly triangular area (A) with dark patches of rock showing through. The light toned ridges (B) are pockets of sand gathered along former beach lines and off the photograph the same feature can be traced for over a mile inland. It is obvious that here is a most desirable food-gathering area only limited by the marsh to the North (C). Subsequent field work by the writer disclosed an extensive camping ground (D) from which it was possible to recover several thousand small implements and flakes. Further to the West, where the reef ceased, very few traces of occupation were found until the next reef area at Point Cook Airfield where the story repeated itself.

In passing it should be mentioned that these reefs are the traces of volcanic activity, being the remains of lava flows. No doubt the disturbances of beach level can be attributed to the same source. These reefs do not appear on the geological maps prepared in the third quarter of the last century and the writer was unable to obtain an adequate Naval chart of the area. From all normal sources of information other than air-photographs they may be regarded as being unknown.

Turning to Malaya two types of site, again beaches, are of interest. Around the shores of Singapore Island and the southern coast of Johore State have been found traces of a culture characterized by partially ground axes, rough flakes and microliths. As yet it has not been possible to assign a period to this culture and the one excavated site awaits publication. So far the sites found by Mr H. D. Collings of the Raffles Museum and the writer have been confined to the beach. Since beaches are inclined to change their outline it follows that it is of some importance to determine which are modern or undergoing change and which appear ancient and static. Furthermore many Malayan beaches are fronted with mangrove—a species of saltwater tree which, under favourable conditions, tends to advance the coastline, leaving the ancient beach behind. But by no means every mangrove swamp is backed with an ancient beach and even where it is the beach does not appear on the one inch or larger map series. In this instance air-photographs provide the only practicable means of amplifying the map. The illustration (PLATE III, top left) shows a typical piece of the southwest Johore coast. The narrow white bands mark the line of the earlier beaches rapidly becoming left behind the advancing mangrove. This area is in an early stage of absorption. In many places in the same area there is now over a mile width of mangrove.

Along the coasts of Northern Malaya and Southern Siam another form of beach ' buildup ' occurs. The continual stream of mud brought down by the rivers forms mud and sand bars along the coast. Initially these appear as a narrow strip of dry land backed by a lagoon. Subsequently the lagoon silts up with vegetable matter and the process repeats itself. Naturally the speed with which the bars buildup depends on a number of factors of which tin mining in the upper reaches of the rivers is one and no general statement can be made. Nevertheless the buildup takes place very rapidly and

significant changes such as the absorption of islands into the mainland will take place in as little as half a century. A knowledge of this feature is of particular importance in dealing with early geographical accounts and the particular problem, to which, incidentally, no direct answer has yet been determined, is to discover which was the beach line at any given period. One possible answer will be the discovery of a series of kitchen middens with datable material. Some middens have been found already and no doubt more will follow in course of time but so far the evidence is insufficient to plot beaches with any certainty. The photograph shows a typical example of beach buildup at Singora in Siamese Malaya. The successive lines of beaches show clearly as dark and light bands, marking sand bar and filled lagoon and a rocky outcrop—an absorbed island—is ringed. Again these beach lines are not shown on the standard large scale map series.

Thus it may be concluded that air-photographs provide an excellent means of locating ' unseen ' archaeological sites provided certain conditions are met. These are :—

1. The knowledge that archaeological remains exist in the area under consideration provided by historical records, casual finds or air-photographs of a key site.
2. Some skill in air-photographic interpretation on the part of the user.
3. Subsequent confirmation by field work.

P. D. R. WILLIAMS-HUNT.

PRIMITIVE HUTS NEAR LONDON

It may be of interest to readers of ANTIQUITY to recall that primitive conical huts, similar to those described by Prof. Stuart Piggott in the March issue, were used by charcoal burners in Epping Forest, less than 40 years ago, and within 12 miles of Charing Cross. I described these in the *Essex Nat.*, vol. XVI, 1910, p. 65, with photographs and scale drawings of the structures. One of these photographs shows how, as a hut fell into decay, the turf covering slid down the sides to form a hut circle. Another photograph was published in *Nature Notes* (the organ of the Selbourne Society) for June 1909.

The main idea of the Epping Forest and the Iberian huts is identical, although there are differences of detail ; for example, in the Epping Forest form the turf-covering was carried to the top of the cone, instead of being confined to the lower part. The first shower of rain filled up the cracks and it was then watertight for months, and stated to be pleasant to live under.

Forty years ago there were only one or two old men who retained the tradition, which is now extinct in the district.

S. HAZZLEDINE WARREN.

BRITISH INSTITUTE OF ARCHAEOLOGY AT ANKARA

The British Institute of Archaeology in Ankara was formally inaugurated on January 15th by H. E. Resat Semsettin Sirer, Turkish Minister of National Education and H. E. the British Ambassador, Sir David Kelly, in temporary quarters at 8 Istiklal Caddesi, Ankara. Members of the Turkish Council of Ministers and the Diplomatic Corps were present, and were received by Prof. John Garstang, director of the Institute, and Mrs Garstang. The Institute is a newly founded body, designed to carry out work similar to that of the other British Schools of Archaeology at Athens, Rome, Jerusalem and Baghdad. It will form a centre of research to which British students of archaeology and kindred subjects such as anthropology and ethnology will go to carry out their work. For this purpose the Institute is bringing together a library and collections of archaeological objects, photographs and squeezes of inscriptions in so far as these are necessary

to supplement the collections in Turkish museums. It is offering this year a scholarship of £300 to a student wishing to carry out researches in Turkey in some aspect of Anatolian archaeology. It is also hoped to offer such a scholarship annually and a fellowship of £350 to an advanced student.

As a contribution to research in the archaeology of Turkey, and as a valuable means of training students, the Institute will undertake, as soon as funds are forthcoming, a programme of excavation of sites in Turkey and of archaeological exploration on the surface. It will also give all assistance possible to approved British expeditions for excavation or other research. For the information of would-be students, the following rough estimate of expenses may be useful: living in Turkey, average £T5 per day for 100 days (say, £50); travelling from Britain and back £120; incidental expenses during stay £30; balance for travel and expenses of research in Turkey £100 (total £300). The Institute is organized under a committee in London, on which there are representatives of the British Academy, the British Museum, the London Institute of Archaeology, the British School at Athens, the Society for Hellenic Studies, the School of Oriental Studies, the Royal Society, the Society of Antiquaries, the Royal Asiatic Society, the Royal Anthropological Institute, as well as official representatives of the Universities of Oxford, Cambridge, London and Edinburgh. The Committee's honorary secretary in London is Miss K. Kenyon, c/o Institute of Archaeology, Inner Circle, Regent's Park, London, N.W. 1.

Reviews

DIE ROEMISCHEN BRONZE- UND KUPFERMUENZEN MIT SCHLAGMARKEN IM LEGIONSLAGER VINDONISSA. *Von* MICHAEL GRUENWALD. *pp.* 151, *pl.* 13. *Kommissionsverlag von E. Birkhaueser und Cie A. G. Basel*, 1946. (Verooffentlichungen der Gesellschaft pro Vindonissa, Band II).

The ancient bronze coin that comes to us out of the soil with the mark of heavy wear plain upon it has its own immediate appeal to our imagination. It is a tiny piece of ancient civilization, seen as it was in working. The appeal is heightened, when the coin bears a stamp or countermark. It is clearly trying to tell us something about its life-story, over and above the fact that it has suffered attrition in the hands of men. The fact that the meaning of these countermarks is often quite obscure only makes them the more exciting.

Dr Gruenwald, in this book, has done something that has never yet been done on a sufficiently ample scale, but something that clearly needed doing. He has studied all the bronze coins of the Early Roman Empire from one particular site, the legionary camp of Vindonissa (modern Windisch), has noted the proportions of countermarked and uncountermarked to one another, and has carefully recorded the occurrence of each separate countermark or set of countermarks. The result is a body of evidence, hitherto unavailable, on which arguments and conclusions can reasonably be based.

Countermarks on Roman bronze coins are severely restricted both in time and place. In time, they belong almost entirely to the 1st century of the Roman Empire. They begin some little way into the reign of Augustus, continue in great force well into the reign of Tiberius, then decline, only to revive under Caligula, Claudius and Nero and the Civil Wars of A.D. 68–69. There is apparently a brief revival under Trajan, and then the end. In place, they seem to be associated mainly with the West of the Empire, and, more particularly, with the Western frontiers. From all this it has been concluded that they had some close connexion with the needs of the Western armies, and this conclusion seems likely to stand. Whatever the meaning of the countermark was, the two conditions that we have outlined must be relevant to it.

The general theory—if one can talk of a ' general theory ' in so little known a subject—has been that the countermark always modifies, in one way or another, the circulation of a coin. The coin may be stamped by a city, other than the city of origin, for its own use. It may be certified as ' good ' or ' acceptable ' by an Emperor later than the one who first issued it. Very rarely, it may bear the mark of a new—reduced—value, enabling it to circulate on, but at a lower rate. All these uses seem to be well attested, and Gruenwald does not attach sufficient importance to them. But, when he takes up arms against the general theory, he is on much stronger ground and must be listened to with attention.

We used to imagine that Roman bronze of the mint of Rome was primarily struck for Rome and Italy and that, if it made its way, as it certainly did, into the Western provinces, it needed to be countermarked to give it authority. So, too, provincial bronze of Lugdunum and city bronze of Nemausus passed beyond its home sphere and was countermarked for use along the frontiers. So we fondly imagined. But Gruenwald has now made a damaging attack on this attractive and comfortable theory. Roman bronze of the mint of Rome, of Lugdunum and of Nemausus is found in mass at Vindonissa. But

of all the coins that ever bear countermarks not quite one in three was actually found countermarked. The bulk of these coins, then, circulated uncountermarked; and it does not appear reasonable to suppose, that the right to circulate was carried over to them from the minority that was countermarked. The time has clearly come for a reconsideration of the whole evidence. Gruenwald's own suggestion—that countermarks were used to turn the ordinary coin into something like a medal, by stamping on it the name of some famous commander—is most ingenious and covers some of the facts that remain unaccounted for under the old theory. And yet we instinctively feel that an alteration of the coin, such as the countermark implies, should affect it in its most vital aspect—its power to circulate. While we feel that this can no longer be explained as simply as we used to do, we are reluctant to abandon the view as completely as Gruenwald does.

One particular observation supplies Gruenwald with a very strong support for his new theory and offers scholars a new and exciting historical possibility. The countermarks IMP AUG and TIB AUG are very often found on the same coin, and IMP AVG often falls on top of TIB AVG, as if to cancel it. If, as we should naturally assume IMP AVG refers to Augustus and TIB AUG to Tiberius, how is this order to be explained? How could Tiberius bear the title of Augustus in the lifetime of his father by adoption? Gruenwald has an answer ready to this question. He looks to the days just before Tiberius went into something like exile at Rhodes and insists that the tension must have been very much more serious than our tradition reports. Tiberius was beyond all question the leading general of the State and he was being pushed into the background to make way for his own step-son, Gaius Caesar. He cannot have failed to resent treatment that was indeed outrageous, or, if he restrained himself, he will have had friends to resent it for him. It was some such friend or friends that placed the countermark TIB AVG on the coins, as if to say that Tiberius, far from meriting dismissal, rather deserved promotion to the same rank as the Emperor. The tension relaxed, when Tiberius bowed to fate and retired. The countermark IMP AVG cancelled the TIB AVG and demonstrated to the troops who the real authority was. This new theory involves a re-reading of our history, and scholars will probably be unwilling to accept it without very careful checking. But it is only fair to admit at once that Gruenwald does supply an explanation of observed facts, that look at first-sight inexplicable.

The book is handsomely produced and equipped with an excellent apparatus of charts and illustrations. It is a pleasant record of the friendly welcome that Switzerland gave to a scholar in exile and of the very good account to which the guest turned that friendly reception.

HAROLD MATTINGLY.

(a) LE NÉOLITHIQUE DU TÉNÉRÉ. *By* G. JOUBERT AND R. VAUFREY. *L'Anthropologie*, tome 50, pp. 325–30.

(b) LE NÉOLITHIQUE DE TRADITION CAPSIENNE AU SÉNÉGAL. *By* R. VAUFREY. *Rivista di Scienze Preistoriche*, vol. 1. pp. 19–32.

(c) LE NÉOLITHIQUE PARA-TOUMBIEN. *By* R. VAUFREY. *La Revue Scientifique*, no. 3267, pp. 205–32.

In these three papers M. Vaufrey describes surface collections of stone implements and in some cases sherds of pottery from various sites (a) in the south central Sahara, (b) in the vicinity of Dakar on the west coast, and (c) between Bamako on the Niger and Aouker to the north, (b) and (c) being collections made over a number of years by the late M. G. Waterlot, who unfortunately left no records of his finds. These papers are complementary to the earlier paper by Mm. R. Delcroix and R. Vaufrey ' Le Toumbien

de Guinée Française ' in *L'Anthropologie* tome 49, pp. 265–312. In this latter paper, which describes finds from caves and rockshelters made by M. Delcroix, M. Vaufrey discussed the origin of the Tumbian culture, which the Pan-African Congress on Prehistory held at Nairobi last year decided must now be called the Sangoan. M. Vaufrey in this paper included, as was done in the original description of the Tumbian, a real new palaeolithic culture, which is characterized by lance-heads and picks, together with polished axes and sherds of pottery from an industry or industries that it is now generally recognized must be much later in date. For M. Vaufrey in this paper the Tumbian includes ' le néolithique de tradition capsienne ', and even this is almost certainly a *mélange* of industries of Capsian descent and a completely different culture, of which core tools often polished before final flaking, polished axes and recessed base arrowheads are characteristic. Because this latter culture was first described in Egypt, it has hitherto been taken for granted that it originated there ; and thus M. Vaufrey holds that Egyptian predynastic influence has penetrated as far as Ténéré and even further west. This view he maintains in his later paper on ' le Néolithique du Ténéré ' written in conjunction with Commandant Joubert. The connexion with the Fayum Neolithic and the Predynastic of Egypt is indisputable, but why should not these Egyptian industries equally well have been derived from the southern Sahara ; (inhabitable during the climatic optimum of Huzayyin, which M. Vaufrey adopts), when advancing desert conditions drove man into the Nile valley ?

In the second paper we have from the Dakar area an industry undoubtedly of Capsian origin, with larger tools of basalt and sherds unfortunately not figured, which may or may not be contemporary with the Capsian industry. Mere description of sherds is not sufficient for satisfactory identification, even if fuller than that given in this paper. But the ' poterie à bords minces . . . ornée . . . de lignes de traits parallèles . . . occasionellement disposée en ondulations serpentant autour du col ' does suggest the pottery from Early Khartoum associated with an industry of Capsian tradition in which microburins are also absent, with larger tools in rhyolite, and bone ' harpoons ' of a type hitherto only known from just west of Ténéré, which were described in ANTIQUITY no. 84, (1947), pp. 172–81. Sherds of pottery, like stone implements, must be handled before any statements as to their connexion with other wares can be made with any confidence. In default of handling, life size photographs of sherds, of a quality similar to those in fig. 9 of the third paper under consideration, are essential. It is most encouraging to find illustrations of pottery of this quality, for much more detailed attention than has been given hitherto must be given to finds of pottery from all over French Africa before its long and complicated history can be worked out, and with that its relation to the wares of neighbouring countries, particularly those in the Nile valley, where alone hitherto it has been possible to give definite dates.

In the third paper M. Vaufrey includes finds that almost certainly differ considerably in date under the new term ' le néolithique para-toumbien ', and he attempts, pending the discovery of stratified sites, to defend the retention of the name ' Tumbian ' for a culture including bi-face implements of palaeolithic type, polished axes and pottery. In this, however, it is to be feared that he is fighting for a lost cause. Had he chosen to defend the retention of ' Tumbian ' as a term to describe a purified Tumbian of palaeolithic age, as Dr L. S. B. Leakey did so ably in his joint paper with Archdeacon W. E. Owen (Coryndon Memorial Museum Occasional Paper No. 1), he would have been on surer ground and undoubtedly would have found supporters, despite the decision of the Congress to substitute the name ' Sangoan ' for ' Tumbian '. But that paper made it quite clear that in Kenya there are neither polished axes nor pottery in the Tumbian.

At Khartoum, too, we have the Tumbian (or Sangoan) of Khor Abu Anga, yet to be published, which may well be, as Huzayyin has suggested to me, a direct descendant of the late Acheulean also found on that site. At Khor Abu Anga there are certainly no polished axes or pottery. At Khartoum we also have the industry of Capsian origin with pottery and bone ' harpoons ' already mentioned, and described in full in a volume now being printed by the Oxford University Press and to appear under the title ' Early Khartoum '. At Khartoum we have another culture, distinct from, and almost certainly later than, the (?) Mesolithic of Capsian origin of Early Khartoum, which has a pottery entirely different from that of Early Khartoum, rare polished axes, and some of the core tools characteristic of the Fayum Neolithic and some Saharan sites, but lacking all developed arrowheads ; and no doubt future work will distinguish other cultures as well. It is however striking that we have already been able to distinguish three different cultures, the ' Tumbian ', the Capsian, and the core industry first noticed and described in Egypt. It is abundantly clear from M. Vaufrey's papers that all these three cultures have played their part in the prehistory of French Africa ; and the time has now come for the scientific excavation of sites—and it is to be hoped that some stratified sites, unfortunately so rare in Africa, the land of sand and erosion *par excellence*, will be found—so that all doubts may be put to rest, and connections with neighbouring territories worked out on the evidence that will thereby be provided. A. J. ARKELL.

1. YNGRE STEINALDER I NORD-NORGE (Instituttet for Sammenlignende Kulturforskning. Ser. B : skr. XXXIX). *By* GUTORM GJESSING. *Oslo*, 1942. *pp.* 525, *fig.* 232, *pl.* IV. *English summary*. 30 *kr.* (*Norwegian*).
2. TRAEN-FUNNENE (Inst. f. Sammen. Kulturforskning. Ser. B : skr. XLI). *By* GUTORM GJESSING. *Oslo*, 1943. *pp.* 234, *pl.* 52. *Paper*. 30 *kr.* (*Norwegian*). *Boards*, 35 *kr.*
3. CIRCUMPOLAR STONE AGE (*Acta Arctica*, Fasc. II). *By* GUTORM GJESSING. *Copenhagen*, 1944. *pp.* 70, *fig.* 19. 10 *kr.* (*Danish*).
4. NORGES STEINALDER. *By* GUTORM GJESSING. *Norsk Arkeologisk Selskap*, *Oslo*, 1945. *pp.* 527, *fig.* 153. *Price not given.*

These four works, which appeared during the war years, have revolutionized our knowledge of the Stone Age, not only in northernmost Europe, but over the entire circumpolar zone of the northern hemisphere. In the first work to be noticed, Gutorm Gjessing, who was recently called to the chair of Ethnology at Oslo, shows that, so far from being an impoverished outlier of the Danish and south Swedish areas, the Arctic Culture of northern Scandinavia belonged to quite a different world and represented in fact a western bastion of an immense culture-province extending right across Eurasia into North America. Gjessing has no difficulty in disposing of the theory that the slate component of the Arctic Culture was a substitute for flint, since as a matter of fact dolomitic flint is common in parts of the extreme north and was used there in Late Glacial or earliest Post-glacial times by the Finnmarkians. Again, he shows with a wealth of illustration how most of the distinctive features of the Arctic Culture of northern Scandinavia are in fact common to a very much larger area, most of which was far removed from any direct contact with the south-west Baltic area. The notion that the north was permeated by civilizing influences from the megalithic area to the south is shown to be quite wide of the mark. Such deep penetrations of the far north as can be demonstrated represent trading expeditions on the part of battle-axe people from Sweden. In truth we are confronted in Scandinavia Neolithic times with two distinct zones of

culture, in certain parts of the south with communities of peasants, elsewhere with groups of hunter-fishers, and cultural exchange between than was reciprocal. The first two-thirds of the book, the evidence for which was assembled while the author was in charge of the archaeological department of the Tromsö Museum (1936–40) are taken up with a detailed description of the Stone Age material from northern Norway, type by type. Over two hundred forms are illustrated and the objects are fully referenced both in relation to museum collections and to the literature. Apart altogether from the general considerations and conclusions, the material collected with such diligent scholarship would alone make the book a notable one.

But Gjessing by no means confined himself to museum and library work. For three seasons (1937–9) he laboured in the field and his second book describes the excavations he made on the small island of Sanda, outermost of the Traen group of islands some fifty kilometres from the Norwegian mainland immediately south of the Arctic Circle. The island of Sanda is only 3 kilometres in area and consists largely of mountainous ground. The prehistoric inhabitants must therefore have depended mainly on fishing and the pursuit of sea-mammals. They lived partly in caves, but chiefly in houses of earth and stone. Excavations in the most prolific cave, Kirkhellaren, show that it was inhabited intermittently from the Stone Age till approximately A.D. 500 Although the fauna has yet to be studied in detail, remains of fish, birds and seals occurred in abundance. It is noteworthy, in view of the notorious conservatism of fishermen, that the bone hooks show no obvious development throughout the long period covered by the cave deposits. The houses fall into two groups, an earlier one comprising oval or circular dwellings, and a later one consisting of oblong structures. The only one of the earlier group to be investigated yielded a lithic industry of Nöstvet type, resembling that from the lowest levels at Kirkhellaren. The oblong houses had immensely thick walls and their roofs were supported by posts set in stone-packed post-holes and arranged in pairs so as to divide the interior into aisles. The finds obtained in Gjessing's excavations suggest that the same type of house continued in use from the late Passage-grave period into the Iron Age. The discovery in a Stone Age hunter-fisher context of houses of the same general type as those used by the Iron Age farmers of Western Norway, Gotland, Öland and other regions is of great interest to students of early house development.

In the *Circumpolar Stone Age* Gjessing further develops his thesis that the Arctic Culture of northern Scandinavia ought to be considered part of an immense spread extending to the Atlantic coast of North America, using a number of maps to sustain his argument. Of the traits which constitute the circumpolar community of culture Gjessing believes some to have spread along the coast, whereas others spread through the forest belt to the south. The former include the skin-boat of *umiak* type developed for hunting sea-mammals, the detachable harpoon used in conjunction with floats or bladders for hunting seals, and the widespread use of slate for knives and projectiles. Among the latter may be numbered curved-back gouges, arrowheads with serrated edges and concave bases, slotted bone handles with flint insets, crescentic flint blades and pottery with pit and comb decoration. It is worth noting in passing that certain of these traits filtered through from the coniferous into the foliaceous forest zone and helped to distinguish the neolithic cultures of Denmark and south Sweden, which thus borrowed from the Arctic Culture of the north. The crescentic blades, though in a form differing from those developed in Denmark, reached the British Isles, and it may be recalled that the present reviewer ventured to attribute the introduction of these to the Peterborough people, whose pottery bears in the pit-ornament another trait of the circumpolar culture.

The masterly character of *Norges steinalder* makes one regret all the more the absence of an English summary. One can only urge that those who make the effort to read it in its native Norwegian will be richly rewarded. The book falls into three main sections, dealing respectively with the pre-Neolithic, Mesolithic peoples, the hunter-fishers who maintained the old traditions alongside the earliest farmers, and the early farmers themselves. Although evidently addressed in part to the non-specialist reader, adequate references are given to the periodical literature. Of the numerous half-tone illustrations the most welcome to foreign readers will be those depicting some of the famous Stone Age sites such as Viste, Skipshellaren or Kirkhellaren. Particularly valuable, also, if a trifle small in scale, are the maps giving the distribution, up to and including the Tröndelag, of pointed and thin-butted celts, battle-axes, flint daggers, flint 'sickles', and bronzes. The index is not only full but well classified. The book itself is far more than a survey of the Norwegian Stone Age material: it discusses in penetrating style some of the basic problems of European prehistory. From the point of view of the European continent as a whole Norway is marginal, but for that very reason its prehistory is instructive, particularly as regards the nature of the Neolithic colonization, the interrelations between farmers and hunter-fishers and the relativity of the Bronze Age as defining a culture-stage. In this book Professor Gjessing has crowned, but not one may hope completed, his great labours for Norwegian, and one may say, for European prehistory. GRAHAME CLARK.

DIE SCHWEIZ ZUR RENTIERZEIT. *By* HANS-GEORG BANDI. *Frauenfeld: Verlag Huber and Co.*, 1947. *pp.* 217, *numerous text figures*, 16 *plates and folding map.* 13.50 *Swiss francs.*

Any book about palaeolithic times which addresses itself to the problem of how men lived in early days, as distinct from how they shaped their flint implements, is to be welcomed. By confining himself to a single culture in a limited area, the Magdalenian reindeer-hunting culture of Switzerland, Dr Bandi has been able in a small compass to sketch in vividly the various aspects of life, in sections dealing with such topics as the food-quest, handicrafts, clothing, art and the rest. The text, which is written with restraint and with a clear knowledge of the sources in straightforward, non-technical language, is illumined by excellent line-drawings and by half-tone plates, illustrating among other things reindeer in their natural habitat. The pages are free from footnotes, but numerous bibliographical entries printed at the end refer to pages and individual lines.

To the specialized reader, the gazetteer, comprising the last quarter of the book, will possibly make more appeal than the text. In the case of each of the thirty-two Magdalenian stations of Switzerland, the essential details are summarized, both as to the site and the finds, and full bibliographies are provided, making the book a handy work of reference.

The Upper Palaeolithic period, which witnessed the first real unfolding of the human spirit, is too seldom appreciated at its proper importance among archaeologists specializing in other fields, largely because it is rarely treated from more than a few limited aspects. In commending Dr Bandi's book one may be pardoned for expressing the wish that a similar, though necessarily much larger work, could be compiled for that *locus classicus* of the Magdalenian reindeer-hunters, the Dordogne. GRAHAME CLARK.